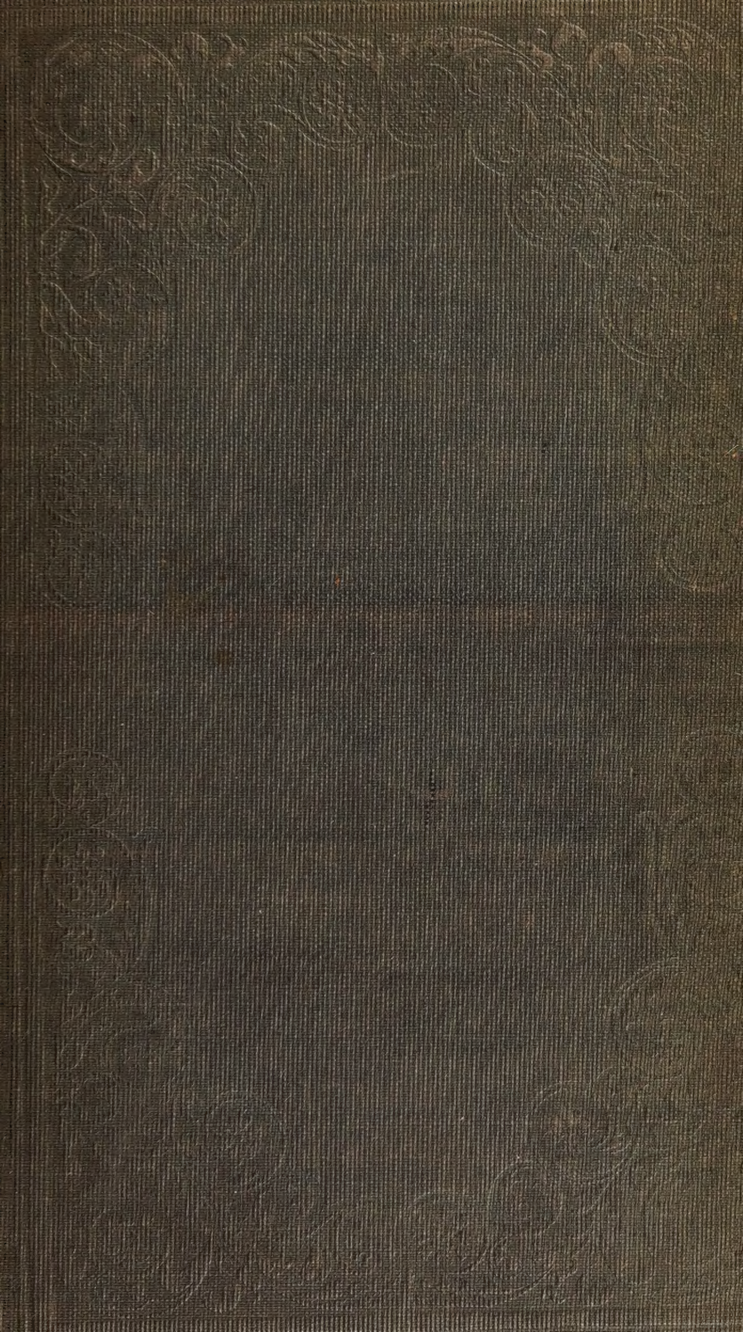

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VOL. XLI. JANUARY—JUNE, 1860.

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THE
RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE,
ETC.

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A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

Gout.—The dietetic and medicinal treatment of gout may be thus summed up:—In the majority of cases all that is necessary is light amylaceous diet, with free use of diluents, but no alcoholic stimulants. Simple alkaline salts with moderate doses of colchicum are required, and, if necessary, an occasional purgative. If there be plethora, the abstraction of a few ounces of blood is advantageous, but, on the other hand, if the vital powers are at a low ebb, sesquicarbonate of ammonia should be substituted for the other salines, and the colchicum altogether omitted. Lithia possesses the remarkable property of forming the most soluble salt known of uric acid; hence the carbonate of lithia is a salt well calculated to be used as a remedial agent in the treatment of gout. (Dr. A. B. Garrod, p. 27.)

Rheumatism, *Acute.*—It is a mistake to suppose that the profuse sweating in cases of acute rheumatism is an eliminative effort of nature: those cases are the worst in which it is most marked and prolonged. Any treatment tending to increase this will prolong the case. Tincture of the muriate of iron is a remedy of much value, when this symptom is excessive. (Dr. Inman, p. 30.)

In acute rheumatism where there is intense suffering, and constant restlessness, give opium freely. If necessary give a grain every hour, until relief to the patient's suffering is obtained. It may be advantageously combined with small doses of the compound colocynth pill. Thus administered it will not be found to "lock-up" the secretions. In fact, it acts favourably as an eliminator, and takes the patient pleasantly ("jucunde") through his disease; and is often followed by a rapid convalescence. (Dr. W. O. Markham, p. 87.)

Colchicum, when combined with opium, acts far more efficaciously than when given alone, the one drug seeming to modify the action of the other. Twelve parts of colchicum wine may be mixed with two of tincture of opium, and twenty drops taken three times a-day. The colchicum wine is more active when made according to the Prussian Pharmacopœia, which directs 150 parts of colchicum seeds to be macerated in 770 of alcohol. This treatment is of no use in old and chronic cases. (Dr. Eisenmann, p. 31.)

Sthenic Inflammations.—Antimony is particularly adapted to arrest pulmonic and renal inflammation; mercury, iritis and peritonitis; chlorate of potash, buccal inflammation; iodide of potassium, periosteal; and colchicum, synovial inflammation. Alkalies internally and applications of lead externally, arrest cutaneous inflammations; mercury and iodide of potassium, syphilitic; and alkalies and their salts, rheumatic inflammations. (Dr. C. H. Jones, p. 381.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Coma and Narcotism.—When a patient is comatose the lungs gradually become more and more congested, effusion takes place into the pulmonary tissue, and accumulates in the bronchial tubes; yet there is no actual paralysis of the respiratory muscles. In death from narcotism the respiratory muscles and tongue are paralysed, but there is neither the rapid effusion nor the great congestion of coma. Now the treatment of these two cases is quite different. In the treatment of the former, venesection is our sheet-anchor, to relieve effusion and prevent its further formation, and to favour the exit of the bronchial effusion the prone position is necessary. Artificial respiration is unnecessary. In the latter, artificial respiration is invaluable, whilst venesection is uncalled for. The prone position is necessary on account of the lingual paralysis. (Mr. W. C. Hunter, p. 53.)

Delirium Tremens.—In the treatment of this disease a successful issue can be obtained without the use of either opiates or stimulants. It suffices to keep the room in a state of tranquillity and partial darkness, giving gently nutritive and easily digestible food, and keeping the bowels open by gentle cathartics. If the patient is yet labouring under the effects of a very recent debauch, an emetic may be given with advantage at the commencement of the treatment. In time the hallucinations will disappear, sleep return, and the patient be restored to health. The results of actual experience in this plan of treatment are favourable. (Prof. Dunglison, p. 55.)

Epilepsy.—The phenomena of epilepsy are owing to the "gradual accumulation and sudden explosion" of nervous energy; consequently, one of the principal agents in treatment is to prevent this gradual accumulation. This object may be attained in two ways viz., "the artificial and the natural." The former consists in keeping up a constant involuntary action of the muscles, by the administration of strychnine internally, and the application of electro-galvanism externally. By these means the attacks can be warded off to an indefinite period. The natural method consists in the constant voluntary exercise of the muscles, combined with very simple diet and pure air. By his temperance in living, by the activity and simplicity of his habits, by the multiplicity of his avocations, and by the hardiness of his amusements and pursuits, the late Duke of Wellington prevented the recurrence of the fits of epilepsy, which appeared in the later years of his life. (Dr. I. Pidduck, p. 49.)

PERIPHERAL OR CUTANEOUS NEURALGIA.—Rapid and permanent relief may frequently be given by the application of the continuous galvanic current. The best apparatus for this purpose is the Pulvermacher chain, as it can readily be applied to any part of the body. (Mr. H. Lobb, p. 57.)

PARALYSIS AGITANS.—In paralysis agitans confined to one limb, benefit may be expected from the application of a continuous galvanic current (direct) to the limb. It will require perseverance, and must be backed up by measures tending to improvement of the general health. An interesting case of this nature is related, in which, after five applications, the involuntary movements completely ceased. (Dr. J. R. Reynolds, p. 51.)

SCIATICA, LUMBAGO, AND OTHER PAINS.—In many cases of local pain, as in sciatica and lumbago, the pain returns as bad as ever after the narcotic effect of the morphine injected passes off, when that drug is injected hypodermically; but when atropine is employed, the pain disappears more permanently. The strength of the solution of sulphate of atropine employed, should be gr. ij. to ℥j. of water; and of this, from ten to thirty minims may be injected at once. Besides the relief of the pain, the other symptoms produced are, slight drowsiness, dryness of the throat, and dilatation of the pupil. (Dr. Cowdell, p. 61.)

TETANUS.—A case of tetanus is related, in which the internal administration of chloroform appears to have relieved the paroxysms. Ten drops were at first given every twenty minutes. (Dr. A. Dick, p. 49.)

Mr. Ellis, in the nineteenth volume of the Medical Commentaries recommends the use of injections of tartar emetic in large doses, a drachm at a time, and repeated two or three successive times, if necessary. Its combination with opium, the sedative power of which it remarkably increases, is recommended. (Dr. Mackenzie, p. 380.)

Woorara.—Up to this time (Dec. 1859) two cases of chronic tetanus in man on the continent, and one in this country, have recovered under the treatment by woorara. When taken by the mouth the action of woorara is extremely doubtful. It may be applied to the wound, or inoculated in the arm. It is brought to this country as an extract, the active principle of which is the alkaloid curarina. Woorara paralyzes the motor and not the sensor nerves, and may be so administered as to destroy entirely the power of voluntary motion without imparing the consciousness of the animal ; and if a poisonous dose of strychnine be given to an animal, woorara may be administered to the extent to paralyze all the voluntary muscles except those of respiration, and by continuing the thus moderated action of the drug until the kidneys have time to eliminate the strychnine, the life of the animal may be saved. (Mr. T. S. Wells, Dr. Harley, p. 44.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Acupressure.—*A new mode of arresting Surgical Hemorrhage.*—With the single exception of torsion, we may safely assert that at the present day ligatures are the means universally employed with the view of arresting hemorrhage. Yet to sew up the outer or external lips of a large surgical wound by metallic non-irritating sutures, and to leave within its depths a series of other ligatures, each of which must, before it can be removed, produce ulceration, suppuration, and gangrene, at the tied arterial points, is a very paradoxical state of matters. To obviate this, Dr. Simpson proposes a new mode of arresting hemorrhage from wounded arteries, which he has tried with perfect success in several operations upon the human subject. The instruments required are some very sharp-pointed slender needles, or pins of passive or non-oxydizable iron, headed with wax or glass, like the hare-lip pins commonly used by surgeons at the present day. By these the arteries are to be compressed for about two days after the operation, when they may be withdrawn, and there will not be any irritating substance within the wound. If the artery be within the flap a needle is passed through the skin down to one side of the vessel, then passed over it, and again re-entering the surface of the wound brought out through the skin at the other side, exactly as a flower is fastened in the lapelle of a coat by an ordinary pin. The amount of pressure can be regulated and increased when required by the acuteness of the angle at which the needle is introduced and again passed out. If the artery be deeply situated, and in the midst of soft textures, two needles may be employed and passed down parallel to each other and at a little distance apart, till the vessel is reached and just passed, then, by changing the direction of the needles to make them converge, and pressing a little further in, the vessel will be compressed between the two. When the vessel is near a bone, it may be compressed against it, and only one needle is required. This mode of arresting hemorrhage is more easy, simple, and expeditious in its application than the ligature, and little or no irritation is caused in the wound. Phlebitis, pyæmia, and other dangerous complications will be much less likely to arise when this mode of arresting hemorrhage is employed. Some will question whether hemorrhage can be effectually arrested by these means, but it is practically found to answer very well, and much less pressure suffices than would a priori be supposed. (Dr. J. Y. Simpson, p. 163.)

EPISTAXIS.—A case of most intractable epistaxis is related by Dr. Davenport of Iowa, U. S., in which almost every conceivable measure seems to have been used without any avail. Plugging had no effect, the blood still forcing its way out. Tannin was introduced, and nitrate of silver injected. At last Dr. D., injected a quantity of the undiluted perchloride of iron with the effect of at once arresting the hemorrhage, but not before alarming symptoms had been produced from loss of blood. (Dr. F. Davenport, p. 101.)

IMPAIRED CONTRACTILITY OF THE HEART.—It is well-known that valvular diseases of the heart, if only slight actual changes have taken place, may continue for years without causing much inconvenience to the patient, providing the contractility of the chambers is not impaired. This applies almost with more force to slight mitral than to slight aortic disease; for the thickness and great elasticity of the endocardium of the left auricle, powerfully supports the muscular wall of that cavity. The first beginning of downward progress of the case is, then, when the muscular power begins to fail, and it is precisely at this stage of the case that digitalis is most useful. It should always be combined with iron. Under its influence the action of the heart becomes slower and more regular, and the pulse fuller. To have the above effect digitalis must be given in very small doses (four or five minims of the tincture); if given in larger doses, the diastole of the heart is prolonged, and we lay the foundation for the supervention of that very condition which it is our object to avoid—dilatation. (Dr. J. Cockle, p. 80.)

FIBRINOUS DEPOSIT IN THE HEART.—There are some cases in which death takes place not from the disease itself, but from fibrinous deposit in the heart—as occasionally in puerperal fever. In many others this event accelerates death, as in pneumonia, pleurisy, and other sthenic inflammations. Death is occasionally seen to follow abstraction of blood or the administration of purgatives in inflammations, owing to fibrine being deposited in the heart, in consequence of its relative increase in quantity to the watery parts of the blood. (Dr. W. B. Richardson, p. 65.)

HEMORRHAGE FROM TONSIL.—An interesting case which occurred at the Marylebone Infirmary, very forcibly illustrates the value of perchloride of iron as a styptic. Hemorrhage had continued from an incision in an inflamed tonsil, till the patient was pale and pulseless. It was speedily arrested by the perchloride. A glass brush should be kept for use with it. (Mr. H. Thompson, p. 172.)

Leech-bites.—A few layers of collodion will rapidly arrest hemorrhage from leech-bites, when too profuse. (M. S. Martin, p. 173.)

Rheumatic Pericarditis.—The great danger in this disease is failure of the heart's powers. This is generally preceded by certain symptoms, as a feeble, intermittent, and irregular pulse, turgescence of the jugular veins, feebleness of the first sound of the heart. Inflammations of this character are much more liable to supervene in weak than in strong constitutions, and must, to be treated successfully, be regarded as signs of weakness. We must be very cautious, in the first instance, not to injure the patient by depletion; and, in the second place not to delay too long the use of stimuli. (Dr. Stokes, Dr. W. O. Markham, p. 86.)

Varicose Veins.—All operations for the cure of varicose veins, which include opening the vein, whether it be done by caustic or the knife, are dangerous, by leading to suppuration within the vein. We have repeatedly alluded to Mr. Erichsen's plan of operating in these cases, and this plan is still followed by him with great success. In his recent cases the operation is thus performed:—A hare-lip pin is passed underneath the vein on one side, and its point brought out on the other; a piece of elastic bougie, about an inch in length, is then laid over the vein parallel to its course. Then, by means of a silken thread twisted over the bougie and under the two ends of the pin, the vein is compressed between the pin and bougie. The principal points to be noted are—to dip the pin sufficiently deeply so as to avoid all chance of transfixing the vein, and not to apply the ligature so tight as to cause ulceration of the parts. The pin should be removed about the tenth day. The injection of perchloride of iron is decidedly more dangerous than the above plan, but must be resorted to when the knots are so large, or so closely matted together that the pin cannot be passed underneath them. (Mr. J. Erichsen, p. 173.)

Nœvus and Varicose Veins.—The perchloride of iron requires to be used with great care, when injected subcutaneously. In a child, it produced sloughing of the tissues down to the mucous membrane when injected into a congenital nævus of the cheek; and in a case of varicose veins, it produced suppuration and much constitutional disturbance. (Mr. H. Thompson, Mr. P. Hewett, p. 172.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Asthma.—In a case of asthma, of considerable severity, the injection of a solution of sulphate of atropine over the seat of the pneumogastric nerve, was followed by great relief. All the usual remedies had been tried without avail. No organic pulmonary or cardiac lesion of any consequence existed. The point selected for injection was over the track of the pneumogastric nerve, between the left sterno-mastoid and the thyroid cartilage. The usual symptoms of belladonna poisoning occurred in a few minutes. This was repeated several times, the unpleasant effects being counteracted by the employment of opium. The spasmodic attacks were arrested, and apparently permanently. (M. Courty, p. 102.)

Croup.—The following plan of treatment is the result of much experience in a purely country practice. At the commencement, give an emetic of pulv. antim. tart.; and if the child is an unusually strong one, a leech may be applied at the same time. Then, for an infant of six months, give a grain and a half of calomel every hour till it purges freely. To a child a year old, give two grains, and to one two years old, even four grains. The stools will become of a greenish colour, something like chopped spinach, and, immediately consequent on this discharge, an alleviation of the symptoms follows. Salivation cannot be produced by this treatment *in children.* The calomel must be given till the disease succumbs. Of course, this mode of treatment is inapplicable in cases occurring in large towns. (Mr. C. Fergusson, p. 104.)

Dr. Richardson states that he has treated croup at its onset with chloroform by inhalation with a success such as he has not seen follow any other remedy. But the effects of the drug must be fully carried out, and sustained for many hours.—(Brit. Med. Journal, April 7, 1860, p. 261.)

Phthisis.—We must cease to regard phthisis as a single specific disease, to be treated on one uniform plan. It is essentially a state of system in which there is excessive waste, or deficient and imperfect formation of tissue. Now this may proceed (amongst others) from two different and opposite causes, viz., excess of acid, or excess of alkali in the system—for a normal alkalinity of the blood is essential to a proper performance of the combustive process, and a normal acidity of the tissues to the proper performance of the formative process. It is well in cases of phthisis to examine the perspiration. If very acid, great advantage will frequently be derived from the administration of a small quantity of alkali; bicarbonate of potass and gentian may be given. If, on the other hand, the perspiration be neutral (it is seldom alkaline) acids are indicated. A case is related in which, with profuse acid sweats, the administration of oils and other remedial means were not productive of any benefit, till the excessive acidity had been neutralized by a little alkali. (Dr. E. Smith, p. 90).

The opinion of medical men on the subject of the utility of hypophosphites in the treatment of phthisis is singularly at variance. Drs. Quain and Cotton, both physicians to a hospital for consumption, find the salts of no use whatever, and patients who had been stationary under their use immediately began to improve under steel and cod-liver oil. Dr. Dickson, of Jersey, has, on the other hand, found this plan of treatment of great advantage, in two-thirds of the cases in which he has tried it (about thirty), and several of his medical friends have told him that the result in their hands has been most extraordinary. Dr. Churchill, in Paris, has followed it extensively with much success. A writer in the Medical Circular, signing himself R. C. H., says that the hypophosphites stimulate the appetite for food, diminish the expectoration and sweating, and, though not curing phthisis, they retard its progress. (Dr. R. Quain, R. C. H., Dr. Dickson, pp. 94, 96, 98.)

The chief differences between the climate of Cairo and that of Madeira, are that the latter is very moist, and the variations of temperature very trifling; whilst the climate of the former is very dry, and the atmospheric variations pretty considerable during the winter months. These two climates are suitable to different classes of cases of consumption. "An acute form of phthisis, with intense fever, frequent short cough, great dyspnœa on exertion, expectoration scanty but often sanguinolent; such are the symptoms which appear to indicate a residence in Madeira: whilst patients in whom the pulmonary disease is running a chronic course, who are subject to exacerbations followed by ameliorations, in whom the expectoration is abundant, not accompanied with hæmoptysis, are very speedily benefited by the dry atmosphere of Cairo." At the very first onset, when the constitution is still strong, there is no difference between the climates of the two places sufficient to make either preferable the one to the other. (Dr. W. Reil, p. 419.)

PNEUMONIA.—The physiological effects of antimony (sickness, nausea, prostration) are quite opposed to its therapeutical action upon an inflamed lung. By giving the medicine in very small doses (1-10 or 1-12th of a grain) along with diffusible stimulants, we obtain the therapeutical action alone, and may, with the greatest advantage, use it in cases of pneumonia in tubercular and debilitated subjects. (Dr. W. T. Gairdner, p. 89.)

Ægophony without Pleuritic Effusion.—Ægophony is owing to vibration in compressed lung, and only differs from bronchophony in being produced by a different degree of compression. It may thus occur in pneumonia without any pleuritic effusion at all. (Dr. Law, p. 98.)

THROAT, *Instrument for Examining the State of.*—In many cases it is extremely difficult, if not impossible, to get a proper view of the state of the throat and fauces, in affections of these parts. To facilitate examinations of this kind, an extremely useful little instrument is made by Mr. Matthews, of Portugal-street. It consists of a small disc of hard steel, very highly polished and attached by a moveable joint to a rod of softer and bendable metal. By this means the tongue can be held down and the light reflected upon the parts. (Mr. P. C. Price, p. 177.)

TRACHEOTOMY.—The tube introduced should be of sufficient size to admit air freely without over-distending the trachea, and it should always be double, the inner tube fitting pretty close, but not secured by any fastening to the outer, so that it may be easily removed for cleaning, or expelled by the efforts of coughing, if obstructed by mucus. (Mr. J. Spence, p. 102.)

Hook for fixing the Trachea.—Dr. Lawton, of Leighton Buzzard, recommends an instrument for fixing the trachea and guiding the knife in the operation of tracheotomy. It is essentially a "hook" with a groove on the concave surface. It may be obtained of Coxeter, of London. (p. 177.)

CANCER OF THE LIVER.—In many or most cases of cancer of the liver in females, a small subcutaneous tumour will be found in one of the breasts. It is situated just beneath the skin and is not usually larger than a pea. This little tumour is pathognomonic of cancer of the liver, and may, if discovered in doubtful cases, materially assist diagnosis. (Dr. Harding, p. 111.)

CHRONIC DYSENTERY OF HOT CLIMATES.—Dr. Oliver, assistant-surgeon to 1st battalion, 60th Rifles, writing from Calcutta, observes, that in the treatment of the chronic dysentery of that country, having tried with various success every remedy which experience could suggest, he finds bismuth, creasote, and copaiba, the three remedies of most value. Bismuth is most adapted to cases in which tenesmus and tormina have ceased, but there still remain colicky pains, over the entire abdomen, the appetite being impaired and abdominal pulsation marked. Creasote is most useful in cases with much irritability of stomach; and copaiba where the disease occurs in strumous patients. The subnitrate of bismuth may be advantageously combined with morphia; and the creasote given in effervescence with tartaric acid and soda, the latter being in excess. The usual dietetic remedies must, of course, be attended to, and full opiates at night form a useful auxiliary. (Dr. W. S. Oliver, p. 110.)

FISTULA-IN-ANO.—Cases of fistula-in-ano are now treated in the Parisian hospitals by injections of strong tincture of iodine, according to the plan followed so successfully by some of our own surgeons. It is a good plan before injecting, to introduce a tent into the bowel to absorb the superfluous liquid and prevent irritation of the mucous membrane. (M. Bonnafont, p. 186.)

HERNIA.—After performing an operation for the radical cure of hernia, it is a mistake to make the patient wear a truss for a long period, for it not only masks the failure of the operation but promotes the absorption of the newly-formed tissues which are depended upon in most methods of cure, for preventing the re-descent of the bowel. Mr. Wood has now performed his operation fourteen times, in all with success. The operation is not a severe one, as the patients are generally well and out of bed in two or three weeks. (Mr. J. Wood, p. 178.)

A new and very simple operation for the radical cure of hernia is the following:—Having invaginated the skin of the scrotum into the inguinal canal, pass a ligature by means of a curved needle along the invaginated portion, through the internal wall of the canal and out though the skin of the abdomen. The needle is then withdrawn leaving one end of the ligature protruding through the opening. By a similar proceeding pass the other end of the ligature by the same opening into the invaginated skin, through the external wall of the inguinal canal and through the skin of the abdomen, withdraw the needle, one extremity of the ligature being left free projecting through this opening. The two ends of the ligature must now be tightly tied together, bringing into conjunction the opposite sides of the inguinal canal and embracing a portion of the superjacent skin. The ligature must be allowed to ulcerate out, and the lymph effused during the process will completely consolidate the parts. (Mr. H. Lee, p. 183.)

Mr. Furneaux Jordan, of Birmingham, has invented an ingenious modification of Wutzer's instrument, by means of which the parts are consolidated by fluid pressure. Thus the pressure is equable, whatever the position or shape of the aperture. The instrument, and its mode of application will be found fully described at p. 185.

Polypus of the Rectum in Children.—During the last two years Mr. Bryant has seen at least twenty cases of polypus of the rectum in children. This affection, though usually considered rare, is in reality of tolerable frequency. It is usually mistaken for piles, and most of the cases had been under treatment for them, of course without any benefit. The great and prominent symptom is hemorrhage from the bowel. Where in a young child this symptom occurs, be sure to examine the rectum. Polypi of the rectum are usually situated about one or two inches up the bowel, and vary from the size of a pea to that of a large nut. They must be removed either by the forceps or ligature. In some cases the polypus was broken off its attachment by working the finger round its pedicle. No bad result has ever followed. Mr. Bryant has never yet seen a child suffering from piles, and believes that cases so described are generally mistaken, and are in reality cases of polypus. (Mr. T. Bryant, p. 334.)

Ulcer of the Stomach.—Ulcer of the stomach may be diagnosed from ordinary dyspepsia by the peculiar grouping of the symptoms. The pain after meals supervenes in different cases at various periods from a few minutes to an hour or two. The character of the pain. is generally severe and lancinating, and is often relieved by position according to the site of the ulcer. It is speedily relieved by vomiting. The countenance is peculiarly worn and suffering. Hæmatemesis must only be regarded as one of a group of symptoms, without the other members of which it loses much of its significance. The treatment which is more beneficial than all others is the adoption of a purely milk diet. The patient must be directed to take as much at each time as he can do, without bringing on the pain. Patients previously worn out and emaciated, will be found to gain strength as well as comfort, under this diet. The irritability of the stomach in these cases suggests the use of nitrate of silver and opium; of each about a third of a grain three times a day, with one-eighth of a grain of extract of belladonna, and if the bowels require it, a little castor oil or henbane and colocynth. During recovery, citrate of iron may be given with advantage. The ordinary diet must be returned to with great caution. (Dr. W. F. Wade, p. 108.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

AMPUTATION.—When amputating at a joint, do not follow the usual practice of removing the articular cartilages, for this plan is based upon the idea that these cartilages will disintegrate and prevent union, whereas this is not found to be the case in practice. (Mr. H. Walton, p. 160.)

Amputation at the Knee-Joint.—Amputation at or through the knee-joint is not resorted to by surgeons as it deserves to be. Patients in whom this operation has been performed have recovered with stumps of the most useful and serviceable kind, much better adapted to support the weight of the body, by proper appliances, than when the stump exists in the thigh itself. In performing the operation, a large anterior cutaneous flap may be made in front of the joint, right down to the insertion of the ligamentum patellæ, which is there reflected upwards, the knife being carried through the joint. A small posterior flap is then made. The patella is retained, forming a round smooth surface to rest upon. Mr. Fergusson makes two flaps of equal length and removes the patella, but none of the articular surface. (Mr. W. Fergusson, p. 145.)

ANCHYLOSIS OF JOINTS.—In cases where, after injuring a joint or the parts adjoining it, anchylosis of the joint follows, forcible flexure is constantly resorted to at the London hospitals, and mostly with success, provided the union has not been osseous. Chloroform will considerably facilitate the operation. (Mr. Brodhurst, Mr. C. Hawkins, Mr. P. Hewett, p. 162.)

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Contractions and Anchylosis of the Knee.—Excluding cases arising from injury, the remainder (including by far the greater number) may, for practical purposes, be divided into those arising from rheumatic and those from scrofulous disease of the joint. In the latter, there is seldom or never bony anchylosis, for the disease occurs necessarily in persons whose systems are weak and altogether below par. On the other hand, rheumatic disease is very liable to be followed by bony anchylosis; in fact this is the ultimate termination to be expected. The treatment of the two cases is very different. In cases of scrofulous origin, infinitely more good can be effected by steady slow mechanical extension, than by any amount of sudden violence, however skilfully employed. In cases of rheumatic origin, it is necessary to forcibly break through the bony adhesions, (which are rarely very strong) and afterwards to gradually extend by mechanical apparatus, as in the former case. (Mr. H. Coote, p. 153.)

EXCISION OF THE KNEE-JOINT.—Be very careful how you excise the knee-joint before the patient has arrived at his full growth; otherwise, even although you may preserve the limb, it will probably fail altogether to grow in proportion to the rest of the body; the result being, that the limb will be not only useless, but an actual encumbrance. (Mr. O. Pemberton, p. 137.)

In excisions of the knee, as in all other cases of excision, the most important thing is that *the whole* of the diseased parts be removed, and where this cannot be accomplished, the operation should not be performed. Periosteal abscesses in the thigh, connected with bare bone, are a positive counter-indication to the performance of the operation. Not so pus in the joint; for in practice some of these cases are found to recover. The general health of the patient ought to be carefully considered, and especially the presence or absence of the tubercular diathesis. The usual H incision, as also the semilunar incision across the joint, above or below the patella, are open to the objection of making unnecessarily large wounds. A better way is to make a straight transverse incision across the front of the joint, removing the patella. A Graham's saw may be used advantageously, and may be so slanted as to cut the end of the femur slightly convex, and of the tibia slightly concave, so as to fit into one another, which, whilst saving the length of the bones, removes their entire surface. Wire sutures may be advantageously used. It is of the greatest consequence that the back part of the knee be well supported after excision, otherwise dislocation is apt to occur from the ends of the bones falling backwards. (Dr. E. Watson, p. 147.)

FRACTURES.—*Pasteboard Splints.*—Pasteboard, or papier maché, may be manufactured into exceedingly light, firm, and inexpensive splints. When wet, the substance may be accurately moulded to the part, and when dry, will retain the form so impressed upon it. A little cotton wool should intervene between the skin and the splints, (which should be divided into two equal halves) and the whole then enveloped in a bandage. This kind of splint was extensively used by the Russian surgeons at Sebastopol. (Mr. Acton, M. Merchie, p. 162.)

Union by the First Intention—Towels, lint, and sponges should not be applied to the raw surfaces of flaps if wished to heal by the first intention, for innumerable little filaments are left adhering to the wound, and though so minute, act very effectually as foreign bodies. This may be proved by the microscopic examination of the early discharges from a wound so handled. Moreover the flaps are generally dusted over with fine bone-dust, from the use of the saw. Another cause of failure arises from the injurious action of the water used (whether warm or cold) upon the blood which lies in the wound, rendering it unfit to undergo those changes through which it should pass in the process of union. (A. M., Edinburgh, p. 170.)

AFFECTIONS OF THE URINARY ORGANS.

Lithotomy.–One of the chief dangers in the operation of lithotomy consists in opening the "hypoprostatic space," or cellular space between the bladder, prostate, and urethra on one side, and the rectum on the other. This space, lying just beneath the recto-vesical pouch of peritoneum is conterminous with the abdominal cavity, and infiltrated urine is liable to pass into the peritoneal sac by osmosis. In the ordinary lateral operation this space is more liable to be opened the more downward the edge of the knife is directed, and in fact it is safer to cut almost directly outwards. In the rectangular staff operation, the knife is liable to be pushed too low, so as to penetrate the substance of the prostate into the lax cellular tissue beyond. the operator thinking the knife is still in the groove of the director. Dr. Buchanan's rectangular staff operation does not seem to have obtained that amount of trial which its merits deserve. It is remarkably easy of performance, and from the safety from hemorrhage, and certainty in the extent of the incisions, followed by excellent results. The staff is rectangular in shape, and grooved on the convex surface, and when introduced, must be held in the median line, and with the angle just so much depressed, that all danger of wounding the bulb is avoided. The operation presents many points of similarity to Allarton's, and is in one or two points superior. The operation has been very fully described in a previous volume of the 'Retrospect,' vol. xxxv. p. 155. (Dr. A. Buchanan, p. 187.)

Median Lithotomy.—Mr. Teale, of Leeds, now performs median lithotomy, *as a rule*, in his practice, making the lateral operation the exception. He observes, that though in children the finger of the operator is the only dilator required, yet in adults instrumental dilatation is necessary. For this purpose Mr. Teale uses a prostatic dilator (see wood-cut, p. 202) which is essentially a modification of Weiss's female urethra dilator. Through the expanded blades of this instrument, he introduces the long lithotomy forceps, and then withdraws the dilator. It is often necessary to touch gently with the knife. the tense perineal fibres, which forcibly resist dilatation. (Mr. T. P. Teale, p. 200.)

Mr. Bowman uses a prostatic dilator consisting of two blades of thin steel, when fitted together having a blunt point, but when introduced, the finger or an ovoid plug can be readily passed in between them, thus entirely avoiding the danger of pushing the bladder before the finger of the operator. The blades are hollowed on their opposed surfaces, and, being very thin, admit of flexion. These instruments, like Mr. Teale's, may be procured of Weiss. A wood-cut will be found at p. 216. (Mr. W. Bowman.)

In this operation Mr. Henry Thompson uses a director hollowed on one side, differing from Mr. Bowman's in having a single blade only. The finger is passed along the director and acts as a dilator. The instrument possesses a probe point which is run along the staff before the latter is withdrawn. (Mr. H. Thompson, p. 217.)

Mr. Teale describes a probe-pointed director essentially similar to Mr. Thompson's—either will answer the purpose. A wood-cut of this instrument will be found at p. 207.

A prostatic dilator is recommended by Mr. Armstrong Todd, on the same principle as the dilator used by him in strictures of the rectum. (See last vol.) It consists of two parallel blades, introduced before the withrawal of the staff, in a state of close approximation, and then gradually separated by means of a screw in the handle. On the seizure of the stone by the forceps, the dilator is withdrawn. Mr. Todd has not yet used his dilator. It will probably be useful in the same cases as Mr. Teale's will be ; but in young children the simpler the operation is made the better, and with the assistance of such directors as Mr. Bowman's and Mr. Henry Thompson's, no better dilator can be wished than the finger of the operator. (Mr. A. Todd, p. 218.)

Instead of using the ordinary curved staff, it is better to employ a rectangular one grooved from about an inch above the elbow, like that recommended by Dr. Buchanan, of Glasgow. The angle rests against the apex of the prostate, and forms a guide to the exact point where to pierce the urethra. The knife used should be straight-backed, and not more than two inches long ; you can then tell to what depth you have entered it. It is better before introducing the finger, to pass a large beaked director along the passage to open up and clear the canal for the introduction of the finger. At this stage, many surgeons having passed a probe as a guide, withdraw the staff; it is, however, better to retain the latter as it is more stiff, and you are less liable to push the bladder before the finger. No dilator is safe except the finger of the operator. Comparing the lateral and median operations—the median has the advantage in there being less danger from hemorrhage, also in its being impossible to open up the pelvic fasciæ with the finger in the median whilst they may be opened by the knife in the lateral. The median has the disadvantage in there being more danger of wounding the rectum and the bulb (seldom, however, of much consequence when in the mesial line); also in there being, especially in young children, a liability to push the bladder before the finger, tearing across the membranous part of the urethra; and, lastly, in the removal of the stone being much more difficult in the median operation. The inner ring of the neck of the bladder is incapable of dilatation more than to admit a calculus an inch in diameter. If stretched more it is lacerated, and we have one of the most dangerous and fatal accidents in lithotomy. (Mr. J. Erichsen, p. 208.)

Reaction of the Blood and of the Urine.—We may look upon the reaction of the urine as a measure of the reaction of the blood. The kidneys regulate the reaction of the blood. When this latter fluid is too alkaline the urine is feebly acid or even alkaline; when the alkalinity of the blood is below par, the kidneys secrete very acid urine. By observing the reaction of the urine, we can always infer the state of the blood and use our remedies accordingly. (Dr. W. Roberts, p. 115.)

Stricture of the Urethra.—The plan pursued at the Westminster hospital is the following: The stricture having been dilated to the size of a No. 3 catheter, the patient is placed under the influence of chloroform, and the stricture-dilator is introduced. This instrument is like a catheter (but consists of two lateral halves fixed on a central stem. When the instrument is fairly in the bladder the spring or collar which connects the blades is relaxed, and a large perforated rod, about the size of No. 10—12 catheter, bevelled off at the end, is fixed on the central axis and forcibly driven down the urethra. The stricture is suddenly dilated and in most cases ruptured. The instrument is now withdrawn, and the stricture kept open by the passage of large catheters. No more tendency to contract exists, than after other and slower methods of treatment. A very large number of cases have been thus treated, and in no case have any bad symptoms followed. The great advantage of the plan is that it saves much time and suffering. (Mr. W. A. Hillman, p. 228.)

Mr. Marshall, of University College Hospital, describes and recommends a new form of instrument for the internal division of stricture of the urethra. (See wood-cut, p. 227.) It is the general shape of an ordinary catheter, but the point is smaller in diameter than the stem, and the middle portion is flattened into a wedge shape, so as to have two cutting edges, yet not so sharp as to divide any yielding structure like the healthy mucous membrane of the urethra, but when it comes to a hard resisting part this is freely divided. Very little force is required, and the use of the instrument is not followed by that amount of hemorrhage which so often results from the employment of an instrument with a sharper edge. (Mr. J. Marshall, p. 226.)

PERINEAL SECTION.—This operation is often one of peculiar difficulty. The best rule for success, is, to cut freely, to keep most carefully in the median line, and to obtain by examination per rectum a clear idea as to the position of the prostate. (Mr. J. Paget, p. 232.)

VARICOCELE.—There is a plan of operation for the cure of varicocele suggested and practised by Vidal, a distinguished French surgeon, which almost uniformly results in an effectual permanent cure. It is as follows:—The vas deferens, readily distinguished by its round cord-like feel, is first separated from the veins, and entrusted to an assistant; next an iron pin, bored with a hole at each end, is passed between the vas and the veins, and brought out, first notching the scrotum with a scalpel at the point of perforation; then a silver-wire, threaded on a needle so constructed that the wire shall follow it without catching, is passed in at the aperture of entry of the needle, and carried between the integument of the scrotum and the veins, the wire is then brought out at the second puncture. Each end of the wire is now passed through the corresponding hole of the pin, which is twisted round and round repeatedly, each turn causing the wire to be rolled around the pin, and so tightened till the veins are firmly compressed between the pin behind and the loop of wire in front. By this means the scrotum is quite free and uncompressed, and there is no danger of arousing inflammation or œdema. The wire should be tightened from day to day, as it causes ulceration in the veins until it has completely cut through, which results usually in about a week or ten days. Meanwhile there is much plastic matter thrown out around the veins, this finally contracts, and obliterates their channels. Mr. Erichsen has followed this plan with success. and suggests a somewhat simpler method dispensing with the iron pin altogether. (M. Vidal, Mr. J. Erichsen, p. 176.)

AFFECTIONS OF THE SKIN, &c.

BURNS AND FOUL SORES.—Condy's fluid, which is a strong solution of permanganate of potash, when properly diluted (1—4 drachms to the pint of water) may be most beneficially employed as an application to all varieties of suppurating and offensive sores, keeping them sweet and clean, and, if sufficiently diluted, producing no pain whatever. The permanganate of potash yields up oxygen with the greatest facility, and it thus acts by destroying the effluvium. It is particularly useful in the case of burns, which may be dressed with lint saturated in a weak solution of this fluid, over which a layer of cotton wool is applied. It is useful also as an application to offensive cancerous sores, and as an injection in uterine carcinoma. (Mr. M. Henry, p. 243.)

Bruises.—The common arnica plant as a remedy for bruises and similar injuries, whether trifling or severe, far surpasses all fomentations and cooling lotions. The most convenient mode of applying it is in the form of lotion—two to six drachms of the tincture to the pint of water. This application will be found to arrest the formation of ecchymosis and thrombus in a wonderful manner. The pure tincture painted over a black eye in the early stage will almost entirely prevent ecchymosis. It probably acts by producing anæsthesia of the cutaneous nerves and exerts some influence on the ganglionic nerves, which surround the blood-vessels and regulate their action. (Mr. M. Henry, p. 240.)

Corns on the Sole of the Foot.—Occasionally a small exquisitely painful corn forms on the fore part of the sole of the foot, depending upon undue pressure from slight contraction of some of the important tendons, that most frequently affected being the tendo-Achillis. The contraction of the tendon may be so slight as to need careful examination for its detection, but so long as it exists the cause of the corn remains, and it will be found that any other measure less than the division of the tendon will be only palliative. The tendon must be divided subcutaneously, and the foot restored to its normal bearings. (Mr. H. Coote, p. 239.)

Eczema of the Hands.—M. Guillot recommends the following ointment: lard 30 parts; subcarbonate of soda, oil of cade, tar, of each from 2 to 4 parts. (p. 235.)

Favus.—In all cases the scabs must first be cleaned away, either by poultices or careful washing, and then some cryptogam-destroyer used. Mr. Startin applies with great success an ointment containing sulphur and the ammonio-chloride of mercury (the latter in the proportion of ℈j to ℥j of lard). Dr. Jenner uses lotions or baths, containing sulphurous acid. Dr. Hughes Bennett endeavours to prevent vegetable growth by the exclusion of atmospheric air, applying with that object cod-liver oil, and covering the scalp with oil-silk. Dr. Neligan employs an ointment of the iodide of lead The treatment adopted must be followed out perseveringly, and not suspended because the disease may appear well for a time. The removal of the hairs by tweezers will materially increase the chance of cure. (p. 234.)

Lupus.—The tendency, in the treatment of this disease, is to rely too much upon local and especially upon caustic applications, and to overlook the constitutional origin of the malady. An interesting case is recorded by Mr. Weedon Cooke, in a young man, following the occupation of an oyster-dredger. The disease had existed seven years, and was very extensive. Though there were no absolute symptoms of scurvy, yet his strumous aspect, and the nature of his occupation, suggested the possibility of that state of blood existing. An ounce and a half of lemon-juice was ordered to be taken three times a day, with meat and porter and green vegetables, and the following lotions and ointment: bichloride of mercury, eight grains; hydrochloric acid, sixteen minims; water, eight ounces; to be applied three times a day as a wash, afterwards covering the parts with zinc ointment. Under this treatment the ulcers entirely healed in three months, and the general health was re-established. Several relapses occurred in the next three or four years, but were speedily checked by the treatment so successful at first. (Mr. W. Cooke, p. 238.)

Small-Pox.—*Prevention of Pitting.*—Why is it, that pitting is comparatively so frequent in the face as compared with other parts of the surface? Some explain this on anatomical grounds, but more probably it is accounted for by the fact that, whilst the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain exposed, and in a dry and heated state. In the more active and non-typhoid forms of the disease, the application of poultices over the face appears to be the surest mode of preventing the consequent disfigurement. This treatment should be commenced at the earliest period, and continued to an advanced stage of the affection. and, if necessary, applied even over the nostrils (which are generally completely obstructed.) By this means three important indications of treatment are fulfilled: air is excluded, local irritation is moderated, and the parts are kept in a permanently moist state, so that the drying and hardening of the scales is prevented. (Dr. W. Stokes, p. 236.)

VENEREAL AFFECTIONS.

Chordee.—The best remedy in this troublesome and painful affection is camphor. It should be given in the fluid form: a teaspoonful of the spirit of camphor may be taken in water before going to bed, and repeated whenever the patient awakes with chordee. In mild cases, one dose for a night or two is generally enough. (Mr. J. L. Milton, p. 247.)

Gonorrhœa.—M. Ricord's abortive treatment, as laid down in his 'Traite Pratique,' consists of rest, low diet, thirty or forty leeches to the perineum, followed by copaiba and *mild* injections of nitrate of silver, and he expressly confines his recommendation of a strong solution of this salt to those cases which begin "without pain, without any sign of inflammation." (M. Ricord, Mr. J. L. Milton, p. 244.)

Gonorrhœa and Gleet.—Copaiba in the treatment of gonorrhœa is not only unnecessary and very nauseous, but is even in some cases injurious. Gonorrhœa is treated most successfully by the free exhibition of the alkaline carbonates, given with the view of neutralizing the acid in the urine, which acts as a source of irritation to the inflamed urethra. As auxiliaries, lead lotion, and elevation of the penis against the abdomen, are useful. The diet must be restricted during the inflammatory stage. After the subsidence of this, and a muco-purulent discharge being left, nothing answers so well as a chloride of zinc injection gr. ij. to the ounce, or even in some cases only one grain. In strumous and dyspeptic persons, however, and in those of dissipated habits, the inflammation is not so high, nor the urine so acid. The best treatment is then the tincture of iron, or sulphuric acid and bark, or gentian, or calumba and the chloride of zinc injection, as in the other cases. In cases of *gleet*, the diet should be good, and beer or wine used in moderate quantities. (Mr. W. Cooke, p. 251.)

Non-Specific Sores.—It is a fault very generally committed, to consider the generality of ulcerations on the genital organs, as of a specific nature, whereas this is very far from being the case. The majority of these cases will be found to get well speedily under the simplest possible treatment, and are not followed by secondary symptoms. Even induration around the sore is no proof of its being syphilitic: this symptom often occurs in strumous subjects. This bears on practice thus, that we must not hurriedly pronounce on the character of a sore, and at once commence a mercurial course, and irritating applications. Even supposing it to be after all specific in its nature, "nothing is lost but a little delay." (Mr. J. Harrison, p. 248.)

AFFECTIONS OF THE EYE AND EAR.

Acute Sclerotitis and Iritis.—In those cases of inflammation of the deeper-seated structures of the eye, attended by agonizing, often nocturnal pain, in which the usual treatment consists in bleeding, blistering, and mercurialization, give morphia freely, $\frac{1}{4}$ gr of the hydrochlorate every third hour, and apply warm fomentations to the eye. By this simple treatment, it will be found that in the majority of cases the disease will rapidly decline; when, by the old plan of treatment, it would have remained for some time. The action of the drug must be attributed to its power of reducing nervous irritability, which may be viewed as the primary cause of the inflammation. (Mr. J. Z. Laurence, p. 262.)

GLAUCOMA.—*Division of the Ciliary Muscle.*—A new operation for the relief of acute glaucoma is proposed, by which removal of a portion of the iris, as in Gräfe's operation, is avoided. Glaucoma depends upon an arthritic condition of the blood, and the bloodvessels sooner or later become diseased, in the same way as the vessels and valves of the heart are affected in arthritic disease. At first, though the coats of the retinal capillaries appear pretty healthy, yet small aneurismal dilatations are visible, these in a more advanced stage giving way and allowing *extravasation* of the contents, clots being visible in the vitreous humour, which itself becomes of a yellowish tinge. Effusion takes place within the eyeball, and by the resulting pressure upon the ciliary nerves and retina, intense pain, and ultimately total blindness is caused. Mr. Hancock has noticed, what has never been previously observed, viz., that the eyeball is constricted, and marked by a circular depression at the point corresponding to the ciliary muscle, whilst the vessels around this part are engorged to an extraordinary degree. The eyeball is elongated in its antero-posterior diameter, and the sclerotica bulges irregularly. To this condition are attributable the ophthalmoscopic and pathological appearances of the bloodvessels observed in this disease. The operative procedure proposed is the following:—a Beer's cataract knife is introduced at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely, for rather more than one-eighth of an inch; the ciliary muscle is thus divided, whilst the accumulated fluid flows by the side of the knife. By the situation and oblique direction of the incision, a free drainage of the fluid is provided for. The iris is but slightly wounded, and the pupil is preserved of its original size and shape, and in its normal situation. (Mr. H. Hancock, p. 253.)

POLYPUS OF THE EAR.—Various modes of treating polypus of the ear are in use. Soft ones are removed in the same way as polypi of the nose, or their neck is ligatured by an instrument contrived for that purpose; these methods, however, are of doubtful value, on account of the narrowness and obliquity of the canal. Often the polypus grows from the membrana tympani, and then, without great care this structure, with the tympanic chain of bones will be destroyed. Mr. Bowman lately commenced the treatment of a case by slow torsion. The tumour which extended from the meatus, was transfixed by a needle and twisted. The next day another twist was given. It was hoped by this means to cause self-strangulation of the tumour, and thus to ensure its complete removal. The result of the case is not given. (Mr. W. Bowman, p. 266.)

MIDWIFERY, AND THE DISEASES OF WOMEN.

Carcinoma Uteri.—The two best deodorizing agents in carcinoma uteri are Condy's fluid, and tar-water. both of course used as injections. Of Condy's fluid, an ounce should be added to a pint of water, and a cupful used as injection thrice daily. For preparing tar-water, the following is the best formula:—Tar, lb. j.; water, conj. j; mix, stirring with a stick for a quarter of an hour. As soon as the tar has subsided, strain the liquor, and keep it in well—closed jars. Higginson's syringe is invaluable in these cases. A tepid solution of soap and water should be injected first, and immediately afterwards, a breakfast-cupful or more of undiluted tar-water. The syringe should have a long vaginal nozzle, that the injection may come in contact with the ulcerated surface. (Dr. T. Skinner, p. 319.)

Excoriated Nipples.—The use of the following preparation is invariably followed by good results. ℞. Acid tannic. gr. xx., glycerinæ, alcohol. aa, ℥j. M. (Dr. Pierce, p. 326.)

Fœtal Auscultation.—The pregnant uterus, like other hollow viscera, has a regular peristaltic motion, or periodic contractions. This can be at will induced by slight manipulation of the abdomen. The uterus will soon be felt hard and resisting, and it is whilst in this state that the fœtal heart can be most distinctly heard. The stethoscope must be placed firmly on the womb itself, and perpendicular to its surface, and in the horizontal line level with the anterior superior spine of the ilium. The left side should first be tried, then the right. (Dr. Druitt, p. 279.)

Galactagogue.—*Castor Oil Leaves.*—Where there is a deficiency of milk in lying-in women give an infusion of castor oil leaves, with some highly nutritious soup, as conger-eel. The effect will be to determine a copious flow of milk. In Boa Vista the leaves are applied to the breasts of any woman, who has previously borne children, at the catamenial period, with the effect of producing a secretion of milk, which can be perpetuated by the simple irritation produced at the nipple by the suction of a child. Thus a wet-nurse may be procured at pleasure. An extract of the leaves given to unmarried women produces intense pain in the breasts, possibly the suction of a child might even in them induce the lacteal secretion.

The leaves, and a tincture and extract prepared from them, may be procured of Mr. Greenish, New Street, Dorset Square, London. (Dr. Routh, p. 380.)

FUNGOUS CONDITION OF UTERINE MUCOUS MEMBRANE.—*Tannin Pencils.*—In many lesions affecting the cavities of the neck and body of the uterus, particularly in the fungous conditions of their mucous membranes, with consecutive hemorrhages, tannin pencils may be advantageously substituted for intra-uterine injections. The formula for their preparation will be found at p. 322. These pencils are passed into the uterine cavity, and there left to dissolve; this is repeated every three or four days. "The fungous state of the mucous membrane progressively disappears, and the hemorrhages are arrested." (Dr. Becquerel, p. 322.)

OVARIAN DROPSY.—*Injection of Iodine.*—The cases in which an ovarian cyst must not be injected with iodine, are, when the evacuated contents are mixed up with inflammatory products (particularly if the inflammation is at all acute), and also where a multitude of gelatinous and semi-solid masses exist in the contents, rendering their complete evacuation impossible; for these masses are coagulated by the iodine, and then act as foreign bodies. With these exceptions, the injection of iodine into an ovarian cyst may be performed with safety to the patient, and in the case of unilocular cysts, already tapped once or oftener, and still free from inflammatory action, with the most hopeful prospects of a full and final cure. But where the cyst is multilocular, that cyst only will be cured which is injected. A succession of cysts may be treated by repeated injections. "On having recourse to this operation, you require to take with you—1st, An ordinary trocar and canula, with which to puncture and evacuate the cyst; 2nd, A long glass tube, with a nozzle, capable of being adapted to, 3rd, A gum-elastic male catheter, with the opening at the end of the tube; 4th, A uterine sound; 5th, Ten or a dozen ounces of tincture of iodine, or of the liquor iodinei compositus: 6th, A piece of lint, and some long strips of sticking-plaster." (At p. 301 will be found a wood-cut of the glass tube used by Prof. Simpson). After introducing the iodine, and freely manipulating the cyst, to ensure the more effectual application of the irritant to the whole surface of the lining membrane of the cyst, *the fluid must be allowed to run out.* It must not be left there as a lasting irritant. The fluid injected may be the ordinary tincture of iodine of the Edinburgh Pharmacopœia (which contains half a drachm of iodine to every ounce of rectified spirit), or the liquor iodinii co., which, though much weaker, does not contain any spirit, whereby all chance of the intoxicating effect of the alcohol on the system is avoided. For small cysts four to eight ounces of fluid may suffice for injection, but for the larger ones eight to twelve ounces may be required to bring the irritant in contact with the whole of the lining membrane. (Dr. J. Y. Simpson, p. 295.)

Ovariotomy.—*When is it to be had recourse to?* It must never be dreamt of where there is organic disease of any other organ, or the constitution of the patient is in any way unhealthy; secondly, when there is strong reason to suspect, as there often is, carcinomatous deposits in the tumour; thirdly, it must not be performed in cases in which there is only a single cyst, or a single large one and the rest very small; until the trial of milder and safer measures has failed. (Dr. J. Y. Simpson, p. 304.)

In cases in which the stump of the ovarian tumour is unusually short, the use of the clamp is liable to be attended with a certain degree of dragging, and in all cases there are certain dangers and drawbacks which render it desirable to discover some simple means to arrest the flow of blood from the cut ends of the divided vessels. Dr. Simpson believes that by the employment of *acupressure* the desired ends will be attained more safely and effectually than by any of the methods previously employed. By means of one or two acupressure needles he proposes to pin the divided pedicle to the abdominal parietes, so as to retain it effectually in position, and at the same time compress the bleeding vessels. The wound must be closed in the rest of its extent with iron-wire sutures, avoiding the peritoneum. (Mr. Spencer Wells advises this also to be included, as there is then less chance of purulent discharges finding their way into the peritoneal cavity.) (Dr. J. Y. Simpson, p. 311.)

Phantom Tumours.—A case forcibly illustrating the value of chloroform as a means of diagnosing accurately the nature of tumours, suspected to be what are denominated "phantom," is related at p. 317. The use of chloroform, in similar cases, was advocated lately by Dr. Simpson, in his excellent lectures on diseases of women, under the head of 'Simulated or False Pregnancy.' (Dr. Ramskill, Dr. Jones, p. 317.)

Premature Labour.—One of the easiest and safest modes of inducing premature labour is that known as Cohen's method. The woman must be placed upon her back with the nates projecting somewhat over the edge of the bed, and the feet supported by two chairs. Then introduce an ordinary elastic catheter through the mouth of the uterus, keeping it close to the anterior wall, so as to pass between the uterine parietes and the membranes. This being accomplished, inject about seven ounces of water heated to 90° or 100° Fahr. After a moment or two, withdraw the tube. A considerable amount of the water will drain away, but the membranes will be effectually separated from a considerable portion of the uterus, and labour will usually follow within the twenty-four hours. (Dr. E. Noeggerath, p. 267.)

PROLAPSUS UTERI.—*Zwank's Pessary.*—One of the most harmless and efficient forms of pessary is the one originally invented by Dr. Zwank, and as modified by Dr. Eulenburg, of Coblenz, may be thus described:—It consists of two lateral halves, each half consisting of a broad, expanded, and hollowed wing, and a descending ramus or handle, at right angles to the wing. When it is introduced into the vagina the wings are closed and the rami expanded; but when fully introduced by means of an india-rubber spring the wings expand and receive the prolapsed womb, whilst the rami close, and by their form assist in retaining the instrument in position. It must be removed at night and washed, and well oiled before introduction in the morning. About four different sizes will be required, the measurements for which, with wood-cuts, will be found at p. 286. (Dr .E. Noeggerath, p. 280.)

VESICO-VAGINAL FISTULA.—Dr. I. Baker Brown, in performing the operation for the cure of vesico-vaginal fistula uses metallic sutures with a separate bar-clamp to each suture. These clamps will be found figured and described at p. 290. By the use of these clamps the duration of the operation is much shortened—a quarter of an hour will suffice in slight cases. There is also no uncertainty whether the whole extent of the edges of the wound are in perfect apposition or not; and moreover, however irregular the opening, its tortuosity can be followed, without the slightest difficulty. He uses three knives specially made for this operation. (Dr. I. B. Brown, p. 288.)

Dr. Battey, of Georgia, U.S., proposes what he denominates "a new principle of treatment and apparatus" in the performance of the operation for vesico-vaginal fistula. This new principle consists in direct pressure upon the approximated edges of the fistula; and the apparatus by which this is attained combines the offices of both a splint or support to the wound, and a compress upon its edges. A strip of flexible sheet lead, five-sixteenths of an inch in width is perforated with small holes on one edge, and on the opposite is notched with a very fine saw. The length of the strip is of course accommodated to the size of the fistulous opening. The edges of the wound having been pared, this splint is applied much in the same way as is necessary in Bozeman's, Baker Brown's, and Simpson's plans of proceeding, slight modifications of course being necessary from the altered form of apparatus. This mode of operating admits of more direct pressure upon the approximated edges of the wound, but otherwise it is essentially the same as the plans previously recommended and acted upon with so much success. Wood-cuts of the apparatus, and a detailed account of the mode of using it, will be found at p. 292. (Dr. R. Battey.)

MISCELLANEA.

Belladonna, *Poisoning by.* — A series of cases related by Mr. Seaton, of Leeds, remarkably bear out the conclusion previously arrived at, as to the mutual counteracting powers of opium and belladonna. At first, vomiting was induced by an emetic, and then small doses of laudanum given until sleep was procured. This treatment was followed by the happiest results, only one case proving fatal, and that was in a subject already diseased from scrofula. "In none of the cases in which delirium was present, were the symptoms alleviated until sleep was obtained; and, after sleep, the patients felt comparatively well. The pupils were widely dilated so long as the delirium continued, and, when sleep was obtained, were either contracted or reduced to the natural size." (Mr. J. Seaton, p. 408.)

Staple Suture.—Mr. Startin recommends the use of a new form of suture, which he calls the "staple suture." It is especially adapted for closing neatly wounds in the skin, also in fissures of the soft palate and hare-lip. It will be better understood from the diagram at p. 373, than by any description.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE FORMS OF CONTINUED FEVER.

By ALEXANDER TWEEDIE, M.D., F.R.S., Physician to the London Fever Hospital, &c.

[It is almost needless to say, that up to within a comparatively recent period, all continued fevers were included in one class, under the generic term *typhus*. At the present time, however, careful observation of the course, symptoms, and pathology of continued fevers, lead us to divide them into several distinct forms. Much has been written upon this subject, but physicians, who have had most opportunities of judging on the subject, agree now in this view of the question. The observations of Dr. Tweedie on this subject are particularly valuable, from his position at the head of the London Fever Hospital, and the great attention which he has for many years paid to febrile diseases. We extract the following from the Lumleian Lectures,, delivered by him, session 1858—9. We must premise, that he divides continued fevers into four classes: 1. Typhus; 2, Enteric, or Intestinal Fever, (a better term than typhoid); 3, Relapsing Fever; 4, Febricula. The term typhus is restricted to one form, characterized by early prostration, more or less prominent affection of the nervous system, but more especially by the invariable absence of any specific lesion.]

1. *Typhus Fever* originates in circumstances tending to impair the essential or vital properties of the blood, mere especially overcrowding, defective ventilation, insufficient nourishment, and hence its prevalence in times of scarcity and famine. Its accession is marked by no special symptoms, but such as occur in many acute diseases—chilliness alternating with heat of skin; quickened pulse, succeeded by muscular prostration; more or less sensorial disturbance; and between the fifth and eighth day a peculiar morbillous-like eruption, not fading on pressure, and persistent; the duration of the fever being about fourteen, and seldom exceeding twenty-one, days. In fatal cases there is no specific lesion, congestion of the internal organs being the only change observed. If there be other lesions they are superadded or accidental.

2. *Enteric or Intestinal Fever.*—This form (known also by the term *typhoid*) is endemic; and supposed to be produced by emanations from organic matter. Its mode of invasion, slow and insidious, differs little from that of the other forms, except that there is almost invariably diarrhœa from the commencement, followed by gurgling in the right iliac fossa, and tympanitic distension. It is characterized, moreover, by an eruption of rose-coloured spots, visible about the eighth day,—often later,—first on the anterior aspect of the trunk, rarely on the face or extremities, coming out in successive crops, and fading or entirely disappearing on pressure; occasionally epistaxis, little comparative diminution of strength, and sometimes sensorial disturbance. This form is often protracted, seldom terminating before the third week, often lasting much longer. After death there is invariably alteration in the solitary and agminated glands of the ileum, which are enlarged, and more or less extensively ulcerated, according to the duration of the fever, with enlargement and softening of the corresponding mesenteric glands, and increased volume and softening of the spleen.

3. *Relapsing Fever.*—In this peculiar form the invasion is sudden, marked by irregular chills or prolonged shivering, succeeded by hot skin, severe pain in the head and limbs, epigastric tenderness, sometimes vomiting (often accompanied with jaundice), and hepatic and splenic congestion. But the chief peculiarity consists in an abrupt cessation of the symptoms from the fifth to the seventh day, so that the patient is apparently restored to convalescence. A few days afterwards, however, generally about fourteen days from the beginning of the attack, there is a well-marked relapse, or recurrence of the symptoms, which, after a profuse sweat, about the third day again disappear, leaving the patient exhausted, though soon recruited by a prolonged and invigorating sleep. This form is rarely fatal, and leaves no special lesion. It appears to be connected with famine and destitution, and to spread by contagion.

4. *Febricula.*—This is a mild variety of fever, characterized by its short duration and the mildness of the symptoms. It seldom lasts more than a few days, sometimes terminating within twenty-four hours, when it constitutes what has been called *ephemera*, or one-day fever. Its cessation is generally preceded by a critical sweat. It is seldom if ever fatal, unless from the supervention of acute local disease, which of itself may destroy life.

Now let me remark that these are not arbitrary but practical distinctions, recognised in our hospitals as well as in private practice, and occurring either sporadically, or as epidemic visitations. The records of the London Fever Hospital afford the most satisfactory mode of showing the comparative prevalence of each of these forms, in and around this metropolis.

ON ENTERIC FEVER.—[This form of fever is distinguished by its invariable anatomical character, specific lesion of the intestinal glands, and corresponding glands of the mesentery. It has been described under various names, as typhoid fever, abdominal typhus, entero-mesenteric fever, gastric fever, ileo-typhus, and enteric fever. The last is to be preferred as being simple, and expressing the prominent destructive feature of the disease. Enteric fever seldom occurs after forty.]

It commences slowly and insidiously, so that the patient is often able to pursue his ordinary avocations, complaining only of undefined indisposition, such as irregular chills, loss of appetite, more or less headache, thirst, pain of limbs, and a degree of langour and lassitude for which he cannot account. In the course of a day or two, these symptoms increase, the pulse quickens, there is less inclination for exertion, the tongue becomes furred, the bowels disposed to be relaxed, sometimes with abdominal pain, the countenance altered in expression, the face at times flushed, and at length, from increasing weakness, the patient seeks the couch, or prefers to keep to bed entirely.

Towards night, there is more or less aggravation of the symptoms, accompanied with restlessness, inducing constant change of posture in the vain hope of finding relief; the sleep is disturbed and unrefreshing, the thirst more urgent, and the heat of the skin more pungent. Sometimes there is vertigo, or singing in the ears, or occasionally slight epistaxis.

Such are the symptoms of the early stage of enteric fever; and though to an experienced observer, they are sufficient to indicate the disease, there may be reasonable doubt as to its nature, until its characteristic eruption appears. This is seldom visible before the second week (generally from the eighth to the twelfth day) after the commencement of the symptoms, when, on close examination, a few small circular rose-coloured spots may be discovered, chiefly on the anterior and posterior aspect of the trunk, seldom on the face or extremities. As the characters of this specific eruption will be presently pointed out, I shall only observe that each spot is perfectly distinct, fades or entirely disappears on pressure, and after remaining visible for three or four days disappears, fresh spots coming out every two or three days, and undergoing a like process of eruption and decline. In this way successive crops of these spots appear until the conclusion of the disease. This characteristic eruption, however,as we shall presently see, is occasionally absent.

If the abdomen be examined, it will be found more or less distended and resonant: a sensation of gurgling in the right iliac fossa, often accompanied with tenderness, being generally perceptible.

The diarrhœa so commonly observed, as one of the early symptoms varies in degree; sometimes it is moderate, sometimes profuse and exhausting, and in some cases accompanied with blood.

If the case be mild and uncomplicated, these symptoms continue without marked variation till towards the middle or end of the third week, when a gradual abatement of the more prominent, especially of the diarrhœa, portends the approach of convalescence. The change, however, is very gradual. The pulse becomes slower; the alvine discharges less frequent and more consistent; the tongue more clean; the thirst abates; the heat of skin and restlessness disappear; the sleep is tranquil and refreshing; there is gradual improvement in strength; while the appetite for food returns, and is often so keen as to require great vigilance to prevent its too early indulgence, and consequent risk to convalescence.

But the progress of enteric fever does not always run so smoothly. On the contrary, when the stage of convalescence is daily watched for with great anxiety, all the symptoms may continue unabated, or some of them become even aggravated. Thus the headache, which usually passes away towards the end of the second week, may persist, and eventually may be followed by delirium, or by somnolence, gradually passing into coma, more or less profound: the diarrhœa may increase, or a sudden discharge of blood from the bowels arise, which, if it do not destroy life, adds much to the danger: or, again, the intestinal ulceration may extend, inducing progressive emaciation and weakness, from which it may be difficult or impossible to restore the patient. Not unfrequently the progress of the fever is interfered with by some form of pulmonary complication. This may be open and easily detected; but in many cases, more especially when there has been early and severe disturbance of the nervous system, it assumes a latent form, and is recognised only by careful auscultation, which may reveal either pleurisy, bronchitis, or pneumonia, more or less diffused. Sometimes, and indeed not uncommonly in severe cases, there may be both cerebral and pulmonary disease co-existing; so that the fever may be said in its progress to have involved the most important internal organs. Under such extensive complications the issue is seldom doubtful.

In proportion to the intensity of the local complication, the general or febrile symptoms increase; indeed, a low form of secondary or symptomatic fever may be said to supervene. The pulse rises in frequency, and is weak and compressible; the tongue becomes dry, brown, shrivelled, and often fissured; the teeth and lips covered with brown or black incrustation; the emaciation progressive; the weakness day by day more marked; the evacuations passed unconsciously; the sacrum, hips, and other parts subjected to pressure, becoming inflamed and passing rapidly into gangrene; or pus may be deposited in different parts, more commonly in the joints.

It is unnecessary to observe that the ultimate issue, under such complications, must depend on the circumstances presented by each case: the degree or intensity of the local complication, and the stamina or power of the patient to contend against such a formidable disease. He may linger from day to day and eventually recover, or death may result from gradual exhaustion, seldom before the fifth or sixth week. Many cases are protracted to a much later period ; indeed, there is scarcely a limit to the duration of complicated enteric fever. Or, lastly, the ulcerative process may gradually destroy in succession the coats of the intestine, and at length perforate the peritoneal covering ; and thus the patient may be speedily cut off. Dr. Jenner notes that when death is the result of the general disease, the fatal termination always occurs before the thirtieth day. Local lesion sufficient to account for death is always found after that date,—proving the natural duration of the general disease to be about four weeks.

On the Pathology of the Ulcerated Bowel of Enteric or Typhoid Fever.—[The first morbid change observed in the intestine, in enteric fever, is slight swelling of the mucous membrane covering Peyer's patches ; to this rapidly succeeds the deposit of typhous matter which soon presents the appearance of a deep yellow or brown sloughy patch ; this is in a short time detached, leaving a cavity or ulcer on the inner surface of the intestine, known by the name of typhous ulcer. The following summary by Rokitansky gives a clear view of the progressive changes of this intestinal lesion :—]

1. We find the lower third of the small intestine to be the seat of the lesion, the number and size of the ulcers increasing as they advance towards the cæcal valve.

2. The form of the typhous ulcer, when it corresponds to the infiltration and detachment of a larger Peyerian patch, is elliptical ; it is round when it corresponds to a solitary follicle, or a rounded patch, or to the partial detachment of a glandular plexus ; it may also be irregular or sinuous, when corresponding to a partial detachment.

3. The size or circumference of the ulcer varies from that of a hemp-seed or pea to that of a half-crown.

4. The patches are placed opposite to the insertion of the mesentery, their long diameter being always parallel to the longitudinal axis of the intestine. The typhous ulcer never forms a zone ; at least Rokitansky has only seen this occur once in many hundred cases.

5. The base of the ulcer is formed by a delicate layer of submucous tissue, which covers the muscular coat, a well-defined fringe of mucous membrane forming the margin.

In regard to the progress of typhous ulcers, there can be no question that the typhous deposit or infiltration affecting Peyer's patches may undergo the process of resolution. This happens in very mild cases in which the duration of the fever is short. There can be as little question that when ulceration has taken place, the ulcers cicatrize or heal.

This reparative process may be seen, in its different stages, in examining the intestine in cases in which death has unexpectedly occurred, either from some intercurrent local affection in the advanced stage of the fever, or during convalescence. When such an opportunity offers, some ulcers are observed to be superficial and not extensive, others are deep and spreading, while in one or more patches (generally near the cæcum) the ulcers appear smooth and polished, and covered with a thin transparent pellicle, continuous with the submucous tissue around the ulceration—an indication that the process of healing, or cicatrization, is going on.

For this important result, however, two conditions are essential; the one, the termination of the local process—that is, of the deposition of typhous matter, and the complete extinction of the typhous dyscrasia; the other, that the powers of the patient are adequate to withstand such a formidable local affection.

When such favourable circumstances concur, the following, according to Rokitansky, is the mode by which the process of cicatrization is effected. The fringe of mucous membrane which lies upon the base of the ulcer gradually connects itself, from without inwards, with the cellular tissue that invests the base, and, uniting with it, becomes paler and thinner. At the same time, the cellular layer becomes whiter and more dense, and finally converted into a serous lamina, the circumference of which is dovetailed between the muscular and mucous coats. The margin of mucous membrane is beveled off in such a manner that the union is imperceptible, while the line of union, as well as the mucous membrane, is so thinned down, that, at last, their villi appear to have been transferred to the serous lamina. The edges finally unite at one or more spots, and coalesce. Instead of the typhous ulcer, we find, in proportion as the above process is effected, a slight depression on the inner surface of the intestine, due to the thinning of the mucous membrane, and the connexion with a thin cellular layer of denser structure; or we find a spot at which the mucous membrane is more firmly attached and less movable, in the middle of which, by oblique light, we may often discover a smooth remainder of the serous lamina, of the size of a millet-seed; or we may discover a spot at which the mucous membrane is more tense, void of plicæ or folds, smooth, less vascular than the surrounding portion, and particularly less villous. Such cicatrices have occasionally been observed thirty years after the attack of fever. It is singular, and characteristic of the typhous ulcer and its cicatrix, that it never induces diminution of the calibre of the intestine.

In examining these ulcers from the cæcum upwards, we find the healing process in different stages. In some, it has just begun; in others, it is going on partially; in others, again, it has been completed: so that the same course is apparently followed in the healing as in the development of this lesion, the cicatrization being either farthest advanced or completed in the patches nearest the cæcum, and in a proportionally less advanced state as we trace the patches upwards towards the jejunum.

Perforation.—In the destructive progress of the typhous ulcer, the mucous, areolar, and muscular coats may be successively laid bare, so that the floor of the ulcer is formed by the thin transparent peritoneum alone. In some cases this membrane is perforated, so that the intestinal contents escape into the peritoneal cavity, giving rise to acute peritonitis.

The perforation is always found in the centre of an ulcerated patch, and most commonly in the lower third of the ileum; generally it is very small, not larger, perhaps, than a pin's head, unless there be a sloughing ulcer, in which case the aperture is large. There may be a single perforation, or there may be two or more.

These perforations happen, as I have already remarked, not in the most severe cases of typhoid fever, but more commonly when the disease is of moderate severity—when the ulceration is proceeding slowly, but surely, as in the cases of atonic ulcers described by Rokitansky.

As to the period of the fever when this lesion usually takes place, it seldom happens before the middle period—towards the end of the second week; but it may occur at any time between the second and the eighth or ninth week.

I have known it to take place more than once, when the patient was supposed to be well advanced in convalescence, walking about and enjoying food. The possibility of such an accident renders it imperative on the medical attendant—fearless of the imputation of sordid motives—to watch with anxious care the period of convalescence in this dangerous disease.

When the convalescence is protracted, indicated by the general and popular phrase, that the patient does not get on—does not mend—does not gain strength—be assured that Nature has not yet finished her work of cicatrization, and that an atonic ulcer has not healed, and is consequently pursuing a non-reparative course, which may end in erosion or perforation of the bowel, and the almost certain death of the patient. There may be even appearances of such improvement in the patient's general condition as to throw the most cautious and experienced practitioner off his guard. The nutritive powers may be fairly active, the patient may be gaining flesh, the alvine evacuations natural, and even formed. But with all these advantageous circumstances, I have known intestinal perforation take place, and destroy life in a few hours.

These, I admit, are exceptional cases, the perforation, in the great majority, happening in the earlier or more acute stage of the fever.

It is not easy to come to a certain conclusion as to the comparative frequency of this lesion. Louis met with it in eight out of fifty-five cases. In the last ten years there have been in the Fever Hospital, as far as I can trace, twenty examples of perforation out of the total admissions (1820). Out of these, *fifteen* were males, and *five* females. The age of the youngest was eleven; of the oldest, forty-five. The preponderance in males is striking. In my private practice, extending over more than thirty years, I have met with it eight times.

On Typhus Fever.—[Typhus is distinguished pathologically from enteric fever by the entire absence of any specific local affection (unless the mulberry rash can be so called). The importance of being able to distinguish between these two forms of fever cannot be over-estimated, as they differ in their symptoms, duration, and in many respects in their treatment. In short,]

Typhus fever is pre-eminently the type of a blood disease—the fever poison acting primarily on the blood, and secondarily on some organs or parts of the body, though after death no trace of structural change can be detected; differing in this respect from enteric fever, in which, as we have seen, there are invariably well-marked special lesions.

It appears, besides, that the two fevers differ in their etiology. Typhus often prevails epidemically, from unknown causes, and is moreover the almost invariable accompaniment of famine and destitution, more especially in over-crowded and ill-ventilated dwellings; and when once engendered, from whatever cause it may have been induced, it spreads by contagion regardless of age, sex, or local circumstances.

Enteric fever, on the other hand, appears to have less of an epidemic, more of a local character; and though its causes are most obscure, it has been supposed to have an apparent connexion with ill-constructed dwellings and malarious exhalations.

[On the first onset of the disease, or during the period of incubation, there are various symptoms, as]

Irregular chills or shivering, nausea, disrelish of food, thirst, giddiness, perhaps slight pain in the head, somewhat accelerated pulse, with languor and muscular aching—these are felt for days perhaps before the fever can be said, in popular language, to be *formed*. As the disease progresses, these sensations become intensified; there is a visible change in the countenance, the patient is agitated and restless, while the symptoms of general disturbance alluded to are more pronounced, and recur in paroxysms of uncertain duration, but chiefly towards evening, or in the night, which is spent in wakefulness, or disturbed sleep.

But these symptoms are common to other forms of fever, and even to acute diseases in general, so that it is by their progress and further development that we are enabled to form our judgment as to the precise nature of the disease they indicate.

1. There is the peculiar distinctive eruption, known by the term *typhus rash:* or, from its resemblance to the efflorescence of measles, it has been called *mulberry* or *morbillous.* When this eruption is present, it indicates at once, to the experienced eye, this form of fever.

A comparison of this rash peculiar to typhus, with the rose-spotted eruption of enteric fever in the living body, will enable you at once to recognise the one from the other.

Let me draw your attention more particularly to this rash.

The typhus eruption appears from the third to the seventh day of the fever, the spots of which it is composed varying in size from a point to three or four lines in diameter, having an irregular outline; sometimes they are few in number, but more commonly they are numerous, the larger spots being formed by the coalescence of smaller ones. At first they have a dusky pinkish hue, fading or partially disappearing on pressure, and when the finger is passed over, they give the sensation of being slightly elevated. After a day or two they assume somewhat of a brick-dust colour, scarcely fading on pressure, the tint gradually becoming more and more dark, and not disappearing when even firmly pressed. Another distinctive peculiarity is, that each patch, or cluster of spots, remains from its first appearance till the termination of the fever, differing from the rash of enteric fever, in which, as I have already pointed out, the spots not only disappear on pressure, but last for a few days only, and are succeeded by a fresh eruption, so long as the fever continues.

In some cases the typhus rash, though well developed, is less prominently marked. Many of the individual spots are pale, undefined, and run into each other, giving the portions of the skin covered by the rash a mottled appearance. There is, however, considerable modification in the colour, as well as copiousness, of this rash, according as the fever is more or less severe, or in some respects, according to the colour and texture of the patient's skin. It will generally be found that the abundance, and especially the deep colour, of this eruption, are in proportion to the severity of the symptoms, so that it would appear that the colour has an obvious bearing on the gravity of the fever.

In regard to the regions of the body on which this eruption appears, when it is abundant, it covers both the trunk and the limbs, coming out first on the anterior aspect of the trunk, and spreading to the extremities, being rarely, however, seen on the face. When less abundant, it is limited to the chest and abdomen, and, as a general rule, it seems to reach its maximum within the first three or four days after it first becomes visible.

It may be inquired, is this distinctive rash always present in typhus? It is occasionally absent. And here statistical facts are valuable in determining the relative proportion of cases, in which, though looked for, it was not found. Of one hundred and fifty-two cases, of which accurate notes were taken and recorded in the journals of the Fever Hospital by Dr. Jenner, this eruption was present in one hundred and thirty-six, and absent in sixteen. It has been generally observed, too, that when the rash is absent, the fever is mild and of short duration, unless some intercurrent or secondary affection alter the character of the disease. Of the sixteen cases in which it was absent, thirteen were between the ages of four and fifteen, and the remaining three between sixteen and twenty-two. It also appeared, that of the whole number (one hundred and fifty-two), seventy-six (exactly a half) were more than twenty-two years old, and twenty-one between the ages of sixteen and twenty-two inclusive. The eruption was, therefore, present in every individual more than twenty-two years of age; it was absent in three, whose ages varied between sixteen and twenty-two; and absent also in thirteen (or about a fourth) of those fifteen years of age and under, and of these thirteen, there was only one whose age exceeded thirteen.

From these details given by Dr. Jenner, he infers, that of one hundred individuals of all ages with typhus fever, we may expect to find the rash absent in twenty-five (or one-fourth) of those under puberty; from fourteen (or about one-seventh) of those under manhood; but from none above twenty-two years of age.

I have remarked that the abundance, and more or less dark colour, of the eruption are generally in proportion to the severity of the fever. Thus, in the Glasgow epidemic, so well and faithfully recorded by Dr. Stewart, it was observed, that when the cases were few, and the fever mild, (the death-rate being one in ten or twelve only,) the eruption, if present at all, was scarcely visible; but when the cases became more numerous, as well as more severe, (the per-centage mortality being higher—from one in eight and a half to one in six and a half,) the eruption was proportionally developed.

In the Edinburgh epidemic (1838-9) the mortality of cases with scanty eruption was one in eight; where the rash was abundant it was one in four; the duration of the fever being also between one and three days longer in the one than in the other.

As it is important to keep in view the distinguishing characters of the typhus eruption, let me restate them:—

1. It consists of a congeries of spots, which cohere in the form of patches on the trunk and limbs (seldom on the face), of a brownish or mulberry hue, giving a diffused or mottled appearance to those parts of the skin on which it comes out; fading, but not disappearing, on pressure; and differing in those respects from the bright rose spots of enteric fever, which are isolated or distinct, and entirely vanish on firm pressure. 2. The typhus eruption is permanent or persistent—that is, it continues from its first appearance until the fever terminates; differing also in this respect from the rose-spotted rash peculiar to enteric fever, which, after running through successive changes, disappears in a few days, and is succeeded by a fresh eruption of spots, which go through the same changes, crop after crop, until the cessation of the fever. 3. Though this eruption may be discovered by careful examination in a large proportion of cases, it is occasionally absent, its absence, however, being rarely observed in adults, more commonly in children; and it will be found that its presence or absence has an important bearing on the character of the fever—its absence more especially indicating a mild disease.

2. *Cerebral Symptoms.*—In the early stage of typhus, *Headache* is

one of the most constant symptoms, indeed rarely absent. It may be so slight as not to attract the patient's notice; but sometimes, for the first week or ten days, it is severe and persistent, after which it generally abates gradually, and finally disappears towards the end of the second week; or, should it continue longer, it comes on at intervals only—in the evening perhaps, or during an exacerbation of the fever.

Delirium.—The intelligence is seldom disturbed in the milder cases. In the more severe, there is transient confusion, soon lapsing into delirium, perceptible at first only on awaking, or in the night, but becoming more constant as the fever advances. The character of the delirium varies; it is sometimes noisy and violent (typhomania), more often low and muttering, with tendency to somnolence; in other cases, it partakes of the character of delirium tremens—the form usually observed in spirit-drinkers, or in persons harassed with care.

This symptom (delirium) is rarely noticed before the seventh or eighth day. It may appear, however, earlier or later; but at whatever period it comes on, it continues till the termination of the fever.

[There is prostration of the whole muscular system; the facial muscles lose their tone, and give a peculiar expression to the face. There are various affections of the organs of sense, as deafness, imperfection of vision, loss of taste.]

Tongue.—In the beginning of the disease, the tongue is covered with thin white mucus, which in its progress becomes more or less brown, thick, and tenacious. In the more grave cases, this incrustation is still darker, becoming at length almost black or fuliginous, while the tongue itself is shrivelled and fissured, and sometimes coated with blood.

The teeth and lips are incrusted with a similar exudation of dry, offensive mucus, or with bloody sordes, which evidently issues from the mucous surfaces.—a kind of local hemorrhage, in fact,—as blood-discs may be discovered on placing a portion of this sordes under the microscope.

With this dry, parched state of the tongue and fauces, there is often some difficulty in thrusting forward the tongue, or in swallowing, especially when the first efforts are made.

Secondary Affections.—The secondary affections that occasionally arise in typhus often become more important than the primary fever, and not unfrequently determine the issue of the disease. They are, however, incidental only, and in no other way are they to be regarded in connexion with the primary or antecedent fever. They are moreover, due rather to local congestion than to inflammation, thus differing from the secondary affections in enteric fever, in which, if the views of Rokitansky and other pathologists of the German school be adopted, there is a deposit of typhous matter in organs—the lungs, intestinal and mesenteric glands, more especially.

[From the earliest times it has been a common opinion, that the period of the subsidence of a fever is accompanied or preceded by a crisis. This, however, is not the case with enteric fever: in typhus fever, on the other hand, a crisis is frequently observed.]

A change in the aspect of a case of typhus, favourable or otherwise, is sometimes preceded by sweating more or less profuse and persistent sometimes by copious deposit of lithates in the urine, by spontaneous diarrhœa, some form of cutaneous eruption, or occasionally by the formation of an abscess. And this theory receives support from comparison with other forms of fever—the periodic and relapsing, for example,—in which there is an abrupt termination of the febrile disturbance after a profuse and apparently salutary sweat.

When we take into consideration, however, the large proportion of cases in which the convalescence slowly, and, as it were, imperceptibly proceeds, without any evident critical event, we cannot resist the conclusion, that, as a general rule, the fever leaves the body without any marked evacuation, and that when a crisis does occur, it is to be regarded as an exceptional, though favourable circumstance.

In examination of the bodies of persons who have died from typhus fever, without secondary or intercurrent local disease, in a considerable proportion, after the most careful search, no trace of disease in any organ or tissue can be discovered. In other instances, the only deviation from the normal state is congestion of internal parts, more especially of the mucous surfaces of organs.

Even when secondary affections have arisen and had an important influence on the severity and progress of the fever, or proved even the more immediate cause of death, they do not always leave corresponding effects on the organs or parts affected; so that it is more than probable that their function rather than their structure has been implicated in the febrile conflict.

There are no special lesions that can be said to be peculiar to this form of fever. The most common, and perhaps the most constant changes are due to congestion of organs, more especially of their mucous surfaces; but that even these changes are not an essential or invariable element of typhus is proved by the fact, that in a large number of bodies, after the most careful examination, no trace of disease in any organ or tissue can be discovered. When secondary affections arise in its progress, as already stated, they are to be regarded as incidental, having, nevertheless, always an important bearing on the progress and result of the fever, and as being not unfrequently the immediate cause of death.

Changes in the Blood.—The blood, which has from remote ages been supposed to undergo marked changes in its physical properties in fevers, has been, in recent times, subjected to chemical analysis. These investigations show that several of its constituent principles become altered, either primarily or secondarily.

I need scarcely mention that healthy blood contains, besides water, four essential constituents—*corpuscles*, *fibrine*, *albumen*, and *certain saline matters*. The fibrine, albumen, salts, and water, constitute the *liquor sanguinis*—a transparent colourless fluid, in which the corpuscles, red and white, are suspended and carried along in the living vessels.

Again, when blood is drawn from the body, as in ordinary venesection, and allowed to remain at rest, it separates into crassamentum and serum.

The *crassamentum* consists of fibrine, in which the red and white corpuscles are blended together with a small quantity of serous fluid.

The *serum* differs from the liquor sanguinis in one respect only—that it does not contain fibrine; it contains, however, albumen, which gives it the property of coagulating by heat; and if a degree of heat sufficient to decompose the animal matter be applied, the residuum becomes converted into earthy and alkaline salts.

In regard to the proportional or mean amount of these constituents, Becquerel and Rodier found that in 1000 parts of healthy blood there are of

Water	791·1
Fibrine	2·2
Corpuscles	127·2
Albumen	70·5
Fatty matters	1·6
Extractive matters, salts, and loss ...	7·4
	1000·0

So much for the constituents of *healthy* blood, the outline of which now given may be useful for comparison with the changes which it has been observed to undergo in fevers. But it is to be understood that the observations to be made have reference to the blood in both forms of fever, (enteric and typhus); the difference in either being trivial.

The blood, in fever, contains a smaller amount of fibrine than in health, and is often deficient also in albumen. Hence, when bloodletting has been resorted to in the treatment, and more especially when performed in the advanced stage, it often coagulates imperfectly, the crassamentum being soft and diffluent, and rarely exhibiting the buffy coat. It is likewise more liable to putrefy than healthy blood, or than blood abstracted in other diseases. This deficiency in the fibrine of the blood in fevers, when uncomplicated, tends to distinguish it from that in acute inflammations, in which there is an increased amount of fibrine, in proportion to the intensity of the disease.

This gradual defibrination of the blood in fevers cannot be ascribed either to bloodletting (if practised in their treatment) or to other antiphlogistic measures, since in other acute diseases those circumstances (bloodletting and other depletives) do not appear to be followed by diminution—nay, rather by augmentation—of the fibrine. It has, moreover, been ascertained, that if a local inflammation spring up in the progress of fever, the amount of fibrine becomes increased, though it is still less than in ordinary inflammation, the febrile condition apparently influencing the quantitative amount of this constituent, when an intercurrent or local inflammation has arisen. Besides, as soon as the period of convalescence arrives, the fibrine begins to increase, and before the prescribed nourishment could be so elaborated as to contribute to this change, and which appears to proceed with advancing convalescence.

With this diminution of fibrine, it has been shown by the experiments of Andral and Gavarret, that in fevers a considerable increase in the corpuscles takes place, forming another point of contrast with the blood in primary inflammations, in which there is a slow but certain diminution in the corpuscles.

It may, therefore, be concluded—first, that in continued fevers, the fibrine gradually decreases in proportion to the duration of the fever, never exceeding in amount the normal standard, but being much more frequently below it ; and, second, that the corpuscles become increased in amount.

It appears, too, by these researches, that the residue of the serum and solid constituents generally, are little, if at all, diminished in amount, notwithstanding the statements made by some writers that, according to the duration and intensity of the fever, the salts of the blood, especially chloride of sodium, become decreased, a notable increase of water at the same time taking place.

On this doctrine,—the supposed origin of fevers in morbid changes in the blood,—I may observe, that when we reflect that the blood, which was aptly termed by Bordeu *fluid flesh*, has the same proximate principles as the solids of the body, that it is organized, and apparently endowed with vitality—the doctrine that it is liable to disease rests on a more certain foundation than mere vague hypothesis. Whether the fever poison acts primarily on the blood, producing changes in its sensible properties, or whether, as some are inclined to believe, the changes are secondary, it is scarcely worth while to inquire. Indeed, we have no data to guide us in this inquiry, except the statements of Dr. Stevens, who affirms that he and other practitioners in the West Indies observed, that in persons exposed to the fever-producing malaria, the blood underwent marked changes, even before the symptoms became developed.

Again, I am inclined to believe, that the different forms of fever arise from distinct poisons, however induced ; that the true typhus poison acts on the blood alone ; that enteric fever is produced by a poison, *sui generis*, which has a compound action on the blood and on the solids, inducing special lesions, with which, as we have seen, this type of fever is invariably associated ; and that it is more than probable that the relapsing fever, to be next described, has also its origin in a distinct poison, though from its comparative infrequency, little or nothing, as yet, has been satisfactorily made out as to its etiology. But I know no department of the pathology of fevers more important and more worthy of further elucidation than their connexion with morbid states of the blood.

Diagnosis.—The importance of forming a correct diagnosis between the two forms of fever—enteric and typhus—cannot be ever-estimated, the one differing from the other in so many prominent circumstances, and more especially, as we shall presently show in their treatment.

In order to arrive at a correct conclusion as to their differential diagnosis, it will be necessary to institute a comparison of the individual symptoms of each form.

1. It has been pointed out, that in typhus fever the accession of the symptoms is more sudden than in enteric fever, in which it is generally gradual, slow, and insidious.

2. The heat of skin is more constant and more marked in the early stage of typhus; during the exacerbations, it is often pungent and accompanied by flushing. In enteric fever, the heat of skin is moderate, or it may be entirely absent.

3. The brain symptoms, especially delirium, intellectual dulness, and stupor, are more strongly and early developed in typhus. In enteric fever, the cerebral symptoms appear later—usually not until after the first week—and slowly and gradually increase in severity.

4. In typhus, the bowels are generally confined, so as to require the occasional administration of aperients. There is seldom diarrhœa, and if the bowels act more frequently than usual, the evacuations are watery. There is, besides, no gurgling on pressing the region of the cæcum, and rarely meteorism. In enteric fever, the evacuations, on the other hand, are generally frequent and watery, and accompanied with griping. The belly is tense, generally inflated, and a gurgling noise may often be detected by pressure over the right iliac fossa.

5. Epistaxis is rare in typhus; not uncommon in enteric fever.

6. Intestinal hemorrhage is very rare, almost unknown, in typhus; frequent in enteric fever.

7. Emaciation is much less marked in typhus than in enteric fever; in the latter when protracted, it is often extreme.

8. The cutaneous rash is quite dissimilar in the two diseases. In typhus it is dark and mottled, often extensively diffused over the skin, unchanged by pressure, and persists throughout the disease. In enteric fever, the rose spots are bright, isolated, slightly raised, fade or entirely disappear on pressure, and are found chiefly on the chest and abdomen. They last for a few days, and are succeeded by a fresh crop, which goes through a similar course until the termination of the fever.

9. The anatomical characters (or lesions) are distinctive. In typhus, Peyer's patches and corresponding mesenteric glands are in their

natural condition. The vessels and sinuses of the brain are more generally gorged with dark blood. The lungs exhibit more frequently evidence of congestion, especially of the posterior and inferior portions, in typhus than in enteric fever.

In enteric fever, the alteration in Peyer's patches and corresponding mesenteric glands form the great distinguishing characteristics of the disease. The spleen is more often enlarged and softened. Ulceration of the pharynx and œsophagus, and softening of the mucous membrane of the stomach, are more common than in typhus.

10. The duration of the two forms may also serve as diagnostic marks. In typhus it is generally less protracted, terminating in recovery or death within the second week, much more frequently than in enteric fever. We have stated that the average duration of typhus does not exceed fourteen or fifteen days; and if prolonged beyond that period, it is generally owing to some secondary affection or complication. In enteric fever, the average duration is longer,—from twenty to thirty days, and extending, as has been shown, in some instances, to forty, fifty, or sixty days, or even beyond.

In children there is more difficulty in determining the diagnosis of the various forms of febrile diseases than in adults. Let me mention a few of the more prominent.

1. Disorders of the gastric system attended with fever are very common, especially amongst the children of the poor, and often simulate enteric fever; or, perhaps, it more frequently happens that the latter is mistaken, and treated as an acute gastric affection by irritating purgatives. If, however, the symptoms that are so peculiar to enteric fever be kept in view, the distinction is less difficult. For example, in enteric fever, the invasion is not sudden, nor can the symptoms be traced to dietetic mismanagement; the prostration is more marked, the child is more restless and agitated, and often delirious in the night; the stools are frequent and watery; the belly tender and inflated; the splenic region dull; and, above all, the characteristic rose spots, if present, and the prolonged duration of the symptoms, are sufficient to indicate that the child is suffering from enteric fever.

2. This form of fever (enteric) may be distinguished from infantile gastric remittent by the irregularity of the paroxysms in the latter; by the absence of rose spots; by the large, often solid evacuations, requiring active aperients for their dislodgement; by the occasional admixture of undigested aliment in the secretions expelled from the bowels; and by decided improvement resulting from a course of treatment that would certainly aggravate, if it did not hopelessly augment, enteric fever.

It is right, however, that I should mention that some physicians of the present day disbelieve in the existence of this infantile gastric remittent fever, without affection of Peyer's patches, which they state to be a constant element in this common infantile disorder. I cannot adopt this conclusion—at least, I am unwilling to do so—until there be better evidence by accumulated observations of the appearances in the intestine, when this form of gastric fever proves fatal, which, I may observe, rarely happens—a circumstance to be regarded as another distinctive character, and showing clearly, to my mind, the non-identity of the two diseases.

3. Another disease of childhood, which it may be difficult to distinguish from enteric fever, is meningitis, and more especially that form which occurs in strumous children—tubercular meningitis. In this affection, the symptoms from the very commencement indicate severe cerebral disease—violent pain in the head, flushing, aversion to light, and as the disease advances, drowsiness, delirium, dilated pupils, and finally deep coma, sometimes alternating with convulsions. There may be sickness, and perhaps vomiting, but the bowels are generally confined. The rose spots, too, so characteristic of enteric fever, are absent.

If we compare these symptoms with those of enteric fever, it will be found that the cerebral symptoms are much less prominent in the latter, at all events in the earlier period; that there is seldom sickness or vomiting; almost invariably diarrhœa; meteorism; often rose spots; dry, fissured tongue; and, when the fever is protracted, exudation of sordes on the lips and teeth; progressive and often extreme emaciation; sloughing of parts subjected to pressure; and death by gradual exhaustion, rarely, however, preceded by convulsions. The duration of the two diseases is also dissimilar. Meningitis terminates either in recovery or death within the third week, while enteric fever may be protracted beyond the thirtieth or fortieth day.

There is less difficulty in distinguishing cases of infantile typhus. The absence of gastric symptoms, the less pronounced cerebral affection, the less pungent heat of skin, the morbillous eruption, if present, the propagation of the disease by contagion, and its comparatively short and definite duration, are sufficient to distinguish typhus fever from other acute disorders of infancy and childhood.—*Lancet, Jan.* 7, *Feb.* 4, *March* 17, 31, *and April* 14, 1860, *pp.* 3, 108, 265, 313, 365.

2.—ON ZYMOSIS.

(Being an abstract of a paper read by Dr. Richardson, before the Epidemiological Society.)

In commencing, the author first considered the process of fermentation as it occurs out of the body. He dwelt on the different views which have been held as to the nature of this process, explaining that they divided themselves into three groups—the vital, the physical, and the chemical. In regard to zymosis or ferment *in* the body, the view generally held was linked with the idea of the vital character of the fermentation process. Having at some length sustained these positions, the author passed on to notice the objections which might be urged against the vital hypothesis, and then presented a reading of the subject which, resting on a basis purely chemical, explained much that was obscure and conflicting. He did not oppose the idea of zymosis; on the contrary, he gave to it a direct affirmative; but he argued in reference to three points:

1st. That the virus exciting the fermentation was simply an albuminous product.

2nd. That the virus was not by its *presence* the cause of the symptoms, but that the symptoms of the epidemics or zymotics were due to the presence of new chemical products resulting from the new chemical changes.

3rd. That the reappearance in some cases of the virus as an excrete was a necessary result, but that the origin of the virus was purely local.

Dr. Richardson next passed on to show that if the views he had advanced were tenable, they would explain many points which otherwise are all in confusion, would link certain diseases to the zymotic which are not at this moment ranged in that list, and would yet, at the same time, reduce the verbal list of zymotics to a few units.

In the first place, he urged that the theory of zymosis, as he explained it, disclosed the reason of the analogy which exists between diseases acknowledged to be communicable and certain other diseases which are not considered communicable, such as dissecting-room cholera. In the next place, it explained the reason why a small, equally with a large, quantity of poison introduced into the body excites the same intensity of effect. Thirdly, the theory suggested an alliance between diseases arising from the absorption of poisons and diseases produced by changes occurring in the body spontaneously, such as rheumatic fever and tetanus. Fourthly, it explained why the majority of zymotic diseases got well of themselves. If the poisons of these diseases were vital, and increased or multiplied in the body, the reproduction would last so long as the material for the continuance of the reproduction were present; but if the process were simply chemical, as he supposed, there was a direct reason why the diseases ran a limited course, inasmuch as chemical changes, having no independent continuance when their causes are removed, cease necessarily after a time, together with the symptoms excited by them during the period of their operation. Lastly, on the theory suggested by the author, an immense field was opened for direct experimental inquiry. Here scientific researches might take two directions: the one towards tracing the actions of known poisons on animals, and exciting diseases analogous to the zymotic diseases after the synthetical method; the other by ascertaining whether diseases so excited could be transferred to subjects previously unaffected.

The point which attracted most interest in the paper was that which had reference to tetanus. This disease Dr. Richardson claimed to be essentially zymotic. The theory of its production in traumatic cases is that the wound, in the process of healing, secretes a special albuminous product, which has the property of a ferment. This substance absorbed into the body excites new chemical changes, and as a product of these there is developed an alkaline or alkaloidal substance, having properties analogous to strychnine. Whether the patient shall or shall not recover depends, therefore, on the circumstance of this produced poison being or not being eliminated from the system by the excretions before the tetanic symptoms excited have progressed to a certain degree. If the symptoms are extended, or are moderate, or are moderated by medicinal means, the poison may be eliminated, and the patient may recover, or the reverse. In regard to tetanus, two experimental questions were open for solution—viz., Will the secretion from a wound in a tetanic patient, introduced by another wound into a healthy animal, excite tetanus? Secondly, Are the excretions, say of the urine, of a tetanic patient capable, like strychnine in solution, of exciting tetanic spasm in a susceptible animal.—*Lancet, Dec.* 3, 1859, *p.* 565.

3.—ON FEVER AND INFLAMMATION.

By William Addison, M.D., F.R.S., &c.

The Fluid or Plasma of the Blood.—The plasma is a very complicated fluid. Its chief constituents are water, fibrine, albumen, colourless corpuscles, volatile matter, fatty compounds, and salts. In its passage through the capillary vessels it permeates all organs and tissues; and, at various points of the circulation, the parenchymatous depurating organs remove from it those substances, which, if retained in it, would injure its qualities. We find that numerous soluble compounds—substances, for instance, which have been taken as food or drink, and poisons received into the stomach—are absorbed into the plasma, whence they may be again eliminated or removed, changed or unchanged by the depurating organs—the organs of secretion.

Many of the common poisons have a local action as irritants, and also a remote or special action upon some parenchymatous organs; other poisons exhibit only the remote or special action. Cantharides has a special action as an irritant to the stomach; it also affects, remotely, through the plasma, the urinary organs. Morphia, taken by the stomach, affects specially the brain, through the plasma. Digitalis, taken by the stomach, affects the heart; strychnia the spinal marrow, through the same medium.

In experiments on animals with prussic acid, it has been found when death took place within a few minutes only, that the odour of the poison could be perceived in the cavities of the body; and again in other experiments, where preparations of antimony were injected into the blood by a vein in the thigh, the metal has been found in the coats of the stomach.

From these and numerous other facts, which it is unnecessary to particularise, we learn that poisonous substances, taken by the stomach, enter the plasma or mingle with the fluid of the blood; and thereby the poisoning, not of all, but of some one parenchymatous organ ensues, determined by the nature of the poison, and the special qualities of the parenchymatous elements of the different organs. But, if we compare the actions of these common poisons received into the body through the stomach, with the action of aërial poisons inhaled by the lungs, important differences will be found. In the former, the symptoms do not begin with fever, nor is any contagious matter generated in the blood; whereas both these phenomena are distinguishing features of the latter. Here, then, we have a ground for our argument, that the two parts of the blood have distinct pathological relations; for poisons taken by the stomach exhibit themselves by symptoms which proceed from some local parenchymatous organ, and they do not cause the production of a contagious matter in the blood; whereas poisons inhaled from the atmosphere by the lungs very often do so. Yet both species or classes of poisons act through the medium of the blood.

In 1832, numerous cases of cholera, then epidemic, were treated by saline injections, thrown into the blood in large quantities, not only without hurt to the patient, but in some instances with great apparent benefit. Among many others, there is a very remarkable case reported in the 'Lancet' (vol. ii., 1831 and 1832), in which five gallons of saline liquid were injected by a vein, in four days.

The first injection was to the amount of nine pounds and a half; it was thrown into the circulation in the space of eighteen minutes. A few hours afterwards an additional ten pounds of the saline fluid were injected, with four ounces of albumen. This was at one of the clock on the morning of the 29th May; at half-past seven the same morning the injection was repeated to the amount of ten pounds, with the addition of ten grains of sulphate of quinine; at half-past eleven the injection was repeated to the amount of ten pounds. On the second of June, at half-past four in the morning, six pounds and a half of the saline fluid were injected, with six drops of a solution of morphia; and on the 19th of June the patient is reported to have left the hospital, well.

If we look around, we may see in all classes of society individuals of various conditions of body. We may contrast the florid complexion with the pale thin face; tissues laden with fat with the spare habit; those who luxuriate in good living with those existing on bread, potatoes, and water. Persons in each of these classes may be following their daily avocations in good health, yet the blood-fluid, we know, cannot be the same in all of them; nor can it be the same or have the same qualities in the same person, if he greatly alter his diet, his food, and drink. In the chemistry of the blood, there are scarcely two analyses of it alike; and the milky serum sometimes seen in blood after venesection has been referred to articles of diet, or to the recency of a meal, which has for a time changed the qualities of the plasma.

These facts support or coincide with our argument that the blood must be regarded in respect to its pathology, as consisting of two distinct parts; and of these, that the plasma, or fluid, holds a position of different or inferior rank to the red corpuscles in the functions of life. It furnishes elements of growth and reparation to its containing vessels, the common tissue; and it is the medium in which the corpuscles swim; but it varies, within certain limits, in qualities and composition, from day to day, according to the nature and abundance of the food and drink; nay, further, it appears that it may vary, in quality and composition, from poisonous substances taken into the stomach; so as to occasion symptoms of poisoning in some local parenchymatous organ, before any of the symptoms of fever appear, and without evidence of the generation of any contagious matter in the blood.

Distemperatures or Dyscrasies of the Plasma or Fluid of the Blood.—There are varieties or changes of health—moods, tempers, and feelings—dependent upon qualities of blood which are temporary, multifarious, and fleeting. They are cured by attention to diet and the excretions; by good living in those who have fared badly, or are half starved; by abstinence and physic in those who have abused the good things of the table. These changes in the moods and feelings of the individual, have all the characters of dependence upon that part of the blood which is quickly replenished and altered by diet—namely, the plasma.

Also there are numerous forms of inflammation which are uncertain and multifarious—dependent upon qualities of blood occasioned by unwholesomeness of diet, privations, and neglect of the functions of the depurating organs—the skin, kidneys, and bowels. Sometimes the simplest wounds fester and ulcerate. Eruptions, pustules, or boils break out in the skin; or the slightest external injury provokes chronic and ulcerative forms of inflammation without fever, comprehended in the terms scrofula and scurvy. For example:—

Sailors at sea are dieted on salt provisions; and in hot climates are accustomed to tramp about the ship without shoes or stockings. Their feet and legs are consequently bitten by mosquitoes, and, when the men have been a considerable time on a salt diet, and in the hot climate, the wounds ulcerate. The ulcerations become larger every day, and will resist all kinds of local treatment, so long as the diet remains unchanged. But, upon a change of diet, and cruising in a cooler latitude, the ulcers heal spontaneously.

Under analogous conditions—of unwholesome or insufficient diet and confinement—fractures in a hospital will remain only partially united: but upon the patient's removal into the country, cure is completed.

When a patient undergoes a formidable surgical operation, and makes a good recovery, the blood is regarded as of normal constitution, free from any taint or distemper. On the contrary, when trifling wounds and accidents degenerate into chronic ulcerations, it is reputed unhealthy. And the unhealthiness or impurity may be limited to the plasma, or that part of the blood from which elements of repair are taken. At all events, it is a question or matter for inquiry, which part of the blood is most in fault in these cases. "A person shall have a sore upon the leg, which is granulating freely," says Mr. Hunter, "when all at once the granulations shall lose their life, and fade away. New granulations may afterwards spring up, and these shall undergo the same process, and so they would continue to go on, if some alteration in the nature of the parts be not produced." That is to say, some alteration in the plasma or fluid of the blood; differences in the nature of granulations being determined by differences in the qualities of the plasma. These examples are sufficient to shew—what indeed is well known—that forms of inflammation, suppuration and ulceration, are produced and kept up by distemperature or unhealthiness of blood, and more particularly of its plasma. And that a depurative operation is often performed on the plasma, by forms of local inflammation, may be argued from the eruption of small-pox. In scarlet fever, that a contagious matter from the blood is discharged by the vascular action in the skin, may be concluded from the properties of the particles of the exfoliating epidermis. And in gout, that offending matter is discharged from the blood by the local inflammation, is also to be concluded from the morbid material deposited in the parts inflamed.

For demonstrative evidence of forms of inflammation dependent on distemperatures or dyscrasies of the fluid of the blood, we may refer to the *chef d'œuvre* of pathological anatomy, the great work of Rokitansky, also to the lectures of Mr. Paget. Actual proof of alterations in the qualities of the plasma may often be obtained by venesection, in cases of inflammation. The speedy separation of the plasma, or sinking of the corpuscles, which occasions buffed blood, indicates a change in the relative qualities of the two parts of the blood. We have recorded the yellowish, greenish, and purplish hues, and the flocculated appearance like curdled soap in the plasma, of buffed blood before its coagulation in different cases of inflammation. And we have repeatedly observed with the microscope differences in elements of the plasma of blood, taken from within the sphere of inflammation, as compared with elements of the plasma of blood drawn exterior to it. It would seem, then, that abnormal qualities of the plasma promote forms of inflammation: and the disposition of the colourless corpuscles of blood to congregate and attach themselves to the coats of the vessels, we may suppose to indicate a way whereby disqualified or abnormal portions of the plasma are transferred to the common tissue for discharge.

In the first part of this lecture, we spoke of the severance of blood-vessels and other bonds of connection between sloughs and the subjacent texture without bleeding. In abscess and ulceration, from thorns and other foreign bodies impacted in the flesh, we showed that great changes take place in blood-vessels without bleeding.

In fact, in all cases of repair, where exudations and new growth appear, the blood-vessels engaged undergo a change from fibrous to corpuscular—their component elements retrograde. This change is exemplified in granulations. the vessels of which bleed upon the slightest touch. We have a perfect conviction that, in places of inflammation, from disordered qualities of the plasma, the coats of the blood-vessels undergo the same changes; for we find that corpuscular elements of the plasma form, not only the coats of new vessels which arise during inflammation, but also supplant the fibrous element of the coats of the older vessels, which must be opened to supply blood to the new ones; and, as the plasma is the part of the blood which plays the conspicuous part in these changes, so it is not difficult to comprehend how it may be relieved of hurtful material in a place of inflammation.

"That sores give rise to very different kinds of pus," says Mr. Hunter, "is very evident to the naked eye, and that the different parts of which the blood is composed will come away in different proportions we can make no doubt; and we find that whatever is in solution in the blood comes away more in one kind of pus than in another." The inference is, that inflammation exercises a therapeutical action in distemperature of the fluid of the blood. Hence, then, inflammation, exudations, granulations, and pus, may arise for two distinct purposes, namely: for the cure of mechanical injury; for the discharge of sloughs and foreign substances from the solid textures; and as a means of cure in distempered plasma.

In injuries to the common tissue, wounds, lacerations, contusions, and fractures, the process of repair comprises inflammation, granulation, suppuration, and ulceration; new blood-vessels are formed without bleeding, and there are discharges of pus. If physical hindrances interpose, the process is protracted and made chronic until they are removed. If they cannot be removed, the life of the person is endangered, and may be destroyed unless amputation be performed.

In distemperature or injury to the quality of the fluid of the blood, by unwholesomeness of diet, privations, or other causes, reactions of the same kind arise for its depuration. These also comprise inflammation, granulation, suppuration, and ulceration; boils, pustules, eruptions, and gout. If hindrances interpose—if the causes of the distemperature comprised in errors of diet, privations, etc., be not abated, the process is protracted and made chronic until they are removed, and in such cases, as in the sailor's before mentioned, to cure the local affections (the ulcers), the qualities of the plasma must be altered or improved by a thorough change of diet or habits of life.

In the one group—injuries to the solid texture (surgical cases)—the reactions arise for the healing of wounds, the discharge of sloughs, dead matter, and foreign bodies. There is direct evidence of what occasions the action, what it accomplishes, and why it is protracted and dangerous. In the other group—disorders of the blood (medical cases)—the influence of the reaction upon the qualities of the fluid of the blood can only be inferred from collateral events; and these, in gout, small-pox, and the other exanthematous fevers, point unequivocally to the therapeutical characters of inflammation.

In perusing such cases as Mr. Lowdell's, we cannot fail of remarking the advantage the surgeon has over the physician, in being able to go safely beyond the things which are seen. At the bottom of fistulous and ulcerated openings in the flesh, beneath an inflamed and thickened integument, and through a thick crust of new-grown bone, he is able to assist, or rather to accomplish the task nature has long been endeavouring to perform. He lays his hand upon the hindrance which has made inflammation chronic, plucks it away, and the person is restored to health. Likewise in other severe injuries, a compound fracture or a crushed joint, the surgeon surveys the whole of the injured parts, and he has time and opportunity, before therapeutical reactions set in—before inflammation, suppuration, granulation, and ulceration have commenced—to form a judgment of the most probable course of events: and, if upon consultation, serious doubts be entertained whether or not the cure should be entrusted to the natural course, he amputates the limb.

The physician is very differently placed. The inflammations he has to deal with arise from qualities in the blood itself; and when he has protracted and dangerous forms to deal with, he cannot lay his hand upon the cause of danger and take it away.

From the facility with which reactions between the fluid of the blood and its containing vessels may be studied in mechanical injuries, has arisen the *doctrine of repair*, daily exemplified in numerous examples.

But reactions of the same kind, from changes in the quality of the blood itself, owing to the difficulties surrounding their explication, have been separated from the ordinary process of repair. Our object will be to accumulate evidence of the propriety of bringing all reactions between the plasma and the common tissues, the process of repair and inflammation, into one—the physiological—category, and to shew that failure of success, in cases of inflammation of blood-origin, no more alters the nature or intent of the action, than does failure of success in necrosis of bone, and in other surgical cases. But further argument upon this matter must be reserved until we have spoken of the corpuscles of the blood.

Of the Red Corpuscles of the Blood.—The development of the red corpuscles of blood is doubtless conducted on the same principle as that of other cells. They take from the plasma, in which they swim, the materials requisite for their growth, and excrete the products that must be consequent on the act of absorption.

"The only hypothesis," says Dr. F. Simon, "regarding the preservation and growth of the blood-corpuscles, is that their increase in bulk is due to a reciprocal action between the young corpuscles and the plasma; and that, as independent organisms in circulation, every change which occurs in them must be accompanied by a simultaneous alteration in the plasma, from which they obtain their nutriment, and into which their excretions must pass." And of the excretions passed into the plasma, those of them which are injurious are withdrawn therefrom by the depurating parenchymatous organs — the secreting parts of the skin, liver, and kidneys.

Now if we treat the red corpuscles as independent organisms, swimming in the plasma, then any matter or substance, whether derived from the plasma or from the atmosphere, which is necessary to their sustenance, to their normal state and functions, may be regarded and spoken of as nutriment. That is to say, we may speak of the corpuscles of the blood as nourished by elements in the air, as well as by elements of the plasma; and, as respecting the matter they excrete or discharge, it is a part only of their excretions which is passed into the plasma: for certainly another part of their excretions is discharged into the air in the form of carbonic acid, by respiration. Let us pause for a moment to consider this matter. The leaves of a plant take in part of the materials of the nourishment of the plant from the atmosphere, and they excrete or discharge into the atmosphere elements that would be injurious to the economy of the plant if retained in it. Also the rootlets of a plant take up other materials of nourishment from the soil, and they excrete into the ground matters which they do not want. In both these examples, in the roots and leaves of the plant, the bodies which perform these offices of absorption and excretion, are microscopic cells—organisms analogous, and in many respects similar, to the blood-corpuscles.

Animals inhale the air and discharge into it matters which are thrown out from the body by respiration; and, in their nutrition by diet, they cast their droppings or excretions upon the herbage, on which they must afterwards feed.

Unicellular animalcules, in a drop of water, derive from it elements of their nourishment, and also they must discharge into it the matter of their excretions. In a *vivarium*, water is the medium sustaining different forms of life, and the fluid is preserved in a fit and proper state, because the excretions of the animal furnish nutriment to the bodies of vegetable life, and because, on the other hand, the excretions of the vegetable furnish elements of nourishment to the animal bodies: the excretions of one department of life furnishing food and necessaries to the other department.

There is, therefore, nothing incongruous in the corpuscles of blood taking part of their nourishment from the air, and discharging into it a part of the matter of their excretions; and in their taking another part of their nutriment from the plasma, and discharging into it another part of their excretions. Nor, again, is there any incongruity in our finding that the plasma, or fluid of the blood, maintains its purity because the excretions of the corpuscles are ingredients upon which the functions of the depurating organs are exerted. Nor, finally, if with Zimmermann and Simon we should regard the fibrine of the plasma as an exuvial matter, or excretion of the corpuscles, is there any incongruity in finding it to be an essential material, used in the maintenance and repair of the common blood-distributing tissue.

As respects matter passing from the corpuscles into the plasma, we are able to show, by some remarkable experiments, which will hereafter be fully related, that molecular and tailed forms of some material substance may be seen issuing out of them and passing into the plasma, without much alteration of their form or colour; and, to any one witnessing the experiment, there can be no difficulty in concluding that the corpuscles, in their natural state, discharge matter into the plasma. It must be confessed that but little can be demonstrated of the vital and depurating processes constantly going forward in the blood of the living person. Nevertheless, in the absence of such demonstration, we are warranted, by the experiment just referred to, and by the undisputed teaching of physiology, in concluding that the corpuscles of blood are sustained in their vital and chemical qualities by the plasma and the air; also, that their excretions are passed, partly into the plasma and partly into the air. And, these properties of the corpuscles being established, there are some broad and acknowledged facts respecting venous blood and urea which claim our attention.—*British Med. Journal, April* 30, 1859, *p.* 352.

4.—ON THE CONDITION OF THE HEART IN TYPHUS FEVER.

By Joseph Bell, M.D., Physician to the Glasgow Infirmary.

[It is to Dr. Stokes that we were first indebted for pointing out the existence of cardiac softening in typhus fever. This will be found in the chapter "On the Condition of the Heart in Typhus Fever," in his work "On Diseases of the Heart and Aorta." Dr. Bell having acknowledged this obligation to Dr. Stokes, sums up an interesting paper on the subject, by the following propositions:]

1. That in numerous cases of typhus, about the fifth, sixth, or seventh day of the attack, the impulse and systolic sound of the heart become feeble and ultimately imperceptible.

2. That these symptoms indicate a morbid alteration in the structure of the muscular tissue of the heart, especially in the walls of the left ventricle.

3. That this alteration resembles the usual changes which result from congestion and inflammation of muscular structure.

4. That the nature of this pathological change requires further examination and research, because the evidences on which the doctrine

of its non-inflammatory origin rest, are not conclusive; the circumstances on which Louis and Stokes have placed reliance being not uniformly present.

5. That the beneficial influence of stimulants does not prove the non-inflammatory nature of the morbid change, because, in asthenic inflammation, a stimulating treatment is always necessary.

6. That whether or not the pathological alteration be owing to inflammation, the softening must be regarded as one of the special secondary effects of typhus.

7. That the proper treatment is to maintain the action of the heart by stimulants.

8. That in cases of cerebral and pulmonary disturbance arising in connection with the symptoms of cardiac softening, a stimulating plan of treatment is indicated.

9. That the presence or absence of the physical symptoms diagnostic of softened heart, may be relied on as affording trustworthy evidence, by which the sthenic or asthenic nature of these cerebral and pulmonary affections can be determined.

From these propositions it follows as a *corollary*, that it is the duty of the physician to devote the strictest attention to the action of the heart, especially as regards its impulse and sounds, throughout the course of every case of typhus.—*Glasgow Med. Journal, Jan.* 1860, *p.* 409.

5.—ON THE NATURE AND TREATMENT OF GOUT.

By Alfred Baring Garrod, M.D., F.R.S., Physician to University College Hospital, &c.

[Dr. Garrod's work on this subject—the result of much patient investigation and extensive clinical research—we have ourselves read with the greatest pleasure. It was so long ago as 1847 that Dr. Garrod first discovered uric acid in the circulating fluid, since which time various papers of his have appeared in these pages. The principal results of chemical investigation of the blood in this disease, are thus stated by the reviewer:]

That healthy blood contains the merest trace of uric acid or urea, so small as to be in general undiscoverable, except by the most minute and searching chemical examination, and not always then.

That, in gout, the blood is invariably rich in uric acid, which exists in the state of urate of soda, and can be separated from it, either in the form of the crystalline salt in acicular needles, or as rhombic crystals of uric acid.

That in acute rheumatism the blood is free from uric acid, or at least contains no more than in health.

That the serum obtained by the action of an ordinary blister yields uric acid when the blood is rich in this principle, except when the blister is applied to a surface affected with gouty inflammation.

That the perspiration seldom contains uric acid; but that, in gout, oxalate of lime may be crystallized from it, as also from the blood.

Dr. Garrod's researches into the nature of the urine in gout exhibit results somewhat at variance with our previous ideas; for, instead of showing an increased secretion of uric acid, they point to a well-marked diminution almost from the commencement of the disease; and the explanation of the discrepancy between the occasional apparent large amount of urates and the deficiency found by analysis seems to arise from the urine being deficient in quantity before an attack of the disease, and at the same time more acid, thus causing them to be precipitated, and become visible to the eye. From some hundreds of analyses, Dr. Garrod states that

"In the earlier stages of acute gout the urine is scanty, and the uric acid, measured by the twenty-four hours' excretion, also diminished; that this acid is thrown out in much larger quantities as the disease is passing off, and that then, amounts even far above the patient's daily average may be excreted."

In the chronic stages of gout, the excretion of uric acid becomes much further decreased, the urea remaining but little affected; deposits of urates are now more rarely seen, and the urine frequently contains a little albumen. Even in the intervals between the attacks, Dr. Garrod has noticed a deficient elimination of uric acid, pointing out that the kidneys have undergone some structural change.

The great fact arrived at by the researches of Dr. Garrod is this—that in every instance gouty inflammation is accompanied by a deposition of urate of soda in a crystalline form; and he considers this a pathognomonic phenomenon, seeing that it is not found after rheumatic nor any other inflammation. Numerous examinations are related of subjects who had had gout in every degree, from the most severe and inveterate form, to those who had only suffered in a single joint; and in every one the peculiar structural change was detected.

[In the portion of the work devoted to the therapeutics of gout, the following paragraph shortly sums up the subject:]

"The diet should be very light, and chiefly amylaceous; diluents freely used, but no alcoholic stimulants allowed, unless in exceptional cases. The medicinal treatment should consist in the administration of some simple alkaline saline, combined with a moderate dose of colchicum: if necessary, purgatives may be given, selected according to the habit and condition of the patient. In the majority of cases, this will be found to be all that is necessary; but in some instances certain modifications may be essential: for example, if there be plethora, the question of the abstraction of a few ounces of blood may possibly arise; and, on the other hand, if the vital powers are at a low ebb, and great vascular and nervous depression exists, ammonia, in the form of the sesquicarbonate, may be desirable, in addition to, or as a substitute for, other salines; at the same time, colchicum should be altogether omitted, or used with the greatest caution. The only application required, in the majority of cases, is cotton wool covered lightly with oiled silk, which forms a protection to the joint; but now and then an anodyne may be advantageously used, and a small blister is occasionally of service."

In chronic forms of gout, Dr. Garrod considers that the following are the indications necessary to be fulfilled:

"First, to treat the chronic forms of gout by less heroic means than those employed in the acute disorder.

"Secondly, to render the blood pure by augmenting the various secreting functions, more especially of the kidneys and skin.

"Thirdly, to restore the power of the digestive organs, which are usually much impaired in chronic gout.

"Fourthly, to attend to the local mischief which the long-continued gouty inflammation induces in the articular structure.

"And lastly, to carefully regulate the diet, and pay proper attention to regimenal means."

He also proposes a new remedy, the *carbonate of lithia*, which he looks upon as probably destined to become a valuable remedy in gout and other affections connected with the uric acid diathesis. As many of our readers may be unacquainted with the properties of lithia salts, we may state that lithia possesses a very remarkable property—namely, that of forming the most soluble salt known of uric acid; and seeing that the insolubility of the acid and many of its salts leads to the formation of gravel and calculus, and probably to the deposition in gout, the salts of this alkali offer the most promising remedy in these affections. Dr. Garrod has already used them somewhat extensively. Unfortunately the known sources of lithia are at present not very abundant, but this difficulty may probably be soon overcome.

[The so-called "rheumatic gout," Dr. Garrod observes, has no independent existence.]—*Lancet, Dec.* 24, 1859, *p.* 645.

6.—*On Lime-Juice in Rheumatic Fever.* By THOMAS INMAN, M.D., Physician to the Liverpool Northern Hospital.—Some time ago, I stated the results of my experience of the value of lime-juice in acute rheumatism; since then I have had abundant corroborative proof of its utility. The last case under my care is so instructive, that it deserves a record.

Mary S., aged 27, was admitted into the Liverpool Northern Hospital, with a severe attack of the above disease, on the 19th of Sept. I ordered her half a pint of lime-juice daily. On my visit on the 23rd, I found her only a trifle better; and on the 26th I found her worse than she was on admission. As this was the first failure, I had met with, instead of altering the treatment, I went into the shop to inspect the lime-juice, of which I was told the supply was fresh, and furnished by a first-rate house (the Liverpool Apothecaries' Company.) I found it colourless, slightly viscous, and, on my tasting it, it seemed to be nothing but lemonade sweetened and acidulated with tartaric or other acid. I directed the pure material to be procured, and the patient to take it as before. After leaving the Hospital, I lost sight of the patent; but I called to hear of her on the 5th of October. I I did not see her, but next day I received a note from the House-surgeon, stating "that the patient got well rapidly after the lime-juice had been changed, and was discharged cured in three days afterwards."

This case proves:—1. That the lime-juice procured at first-rate houses is not always to be depended upon. 2. That lime-juice does not act simply because it is an acid. 3. That lime-juice may have been undervalued, because those employing it have not had the pure material. 4. That the treatment of acute rheumatism by this drug is not equivalent to leaving the disease alone.—*British Medical Journal, Oct.* 29, 1859, *p.* 865.

7.—*On Acute Rheumatism.* By Thomas Inman, M.D.—[The patient, a man aged 34, was admitted into the Liverpool Hospital with the usual symptoms of acute rheumatism. Severe pain in the joints, excessive but not very odorous perspiration. Lime-juice, freely administered, had no marked effect. Profuse sweating continued for a fortnight, at which period twenty minims of the tincture of sesquichloride of iron were given, three times a-day: improvement immediately began, the sweating and pain abated, and in four days the skin was natural. The patient was soon after able to get up. After detailing the above case, the author offers the following remarks:]

This case illustrates many points:—1. It corroborates what I have before noticed, that those cases of acute rheumatism are the worst in which the perspiration is most profuse. 2. It is the first in which I have known lime-juice to fail. (I must note, however, that I had a similar case some years ago at the Northern Hospital, where I administered lime-juice for three weeks before it effected any apparent change.) 3. It shows that great improvement may occur with a diminution of the perspiration; and, consequently, thut the cutaneous secretion is not an eliminative effort of nature, which we ought necessarily to encourage. 4. Coupling together the facts, that in acute rheumatism the blood is as poor in globules and as rich in fibrine as it is in consumption, Bright's disease, and anæmia; that the cases in which the fever is the worst, are those in whom there is some hereditary or acquired constitutional debility; that the intensity of the disease is augmented and the complications increased by venesection, salivation, and purgation; and that such debilitating treatment prolongs convalescence, if it do not actually prevent it; must we not conclude that the sweating in this disease is more analogous to that in consumption and ague than it is to anything else? 5. Referring to a preceding paper of mine (see Vol. xl. p. 361) upon the influence of vitality upon the secretions, may we not ask whether the well known sour smell of rheumatic sweating is not due to its rapid decomposition *after* it has become external to the skin; and consequently, that it is no more proof of the elimination of lactic acid, &c., than an ammoniacal smell from the bed-pan is a proof that the kidney and bowels are eliminating ammonia from the system? I certainly can recall instances in which the perspiration has smelt rheumatic, when the sole complaint was debility, and conversely, I have known many cases of mild rheumatic fever where the perspiration did not smell sour.—*Brit. Med. Journal, Nov.* 12, 1859, *p.* 906.

8.—ON OPIATED COLCHICUM-WINE IN RHEUMATISM.

By Dr. Eisenmann, of Würzburg.

Dr. Eisenmann first states the wide application which he gives to the word "Rheumatism," denoting by it every affection which may arise in the healthy system, independently of any specific cause, from exposure to cold. "By exposure to cold I do not understand merely the effect produced by the contact of cold water or cold and humid air with the external integument, but also that which takes place when cold and damp air penetrates into the lungs, or when cold water is taken into the stomach, the temperature of the body having been raised by exercise." This view is justified by the facts—1, That cold gives rise to the most various affections of the nervous and vascular systems; 2, That these various affections may become, by metastasis, transformed into each other; 3, That they communicate to the economy a marked predisposition to affections of the same nature, such predisposition being increased with each reproduction; 4, And that they yield to the same treatment, whether they show themselves under the form of neuroses or of vascular affections. Under the title of rheumatic inflammation, therefore, the author ranges all inflammatory affections of the heart, lungs, pleura, peritoneum, kidneys, serous membrane of the liver, &c., when these are not due to any specific cause, and treats them in the same manner as acute articular rheumatism.

The means which beyond all others he has found of efficacy in the treatment of rheumatism is a mixture of colchicum and opium, the colchicum acting far more efficaciously when so combined, and then not giving rise to the half-poisonous effects which often attend its use when given alone. Neither the one or the other substances will produce alone the advantageous effects which result from their union. Dr. Eisenmann speaks not only from his own large experience, but from that of many of his medical friends, among whom "Eisenmann's Drops" have acquired a great reputation. These consist of 12 parts of colchicum-wine, and 2 of tincture of opium, 20 drops being taken three times a day. Instead of preparing the colchicum wine with sherry as he formerly did, he now makes it according to the formula of the Prussian pharmacopœia, which directs 150 parts of colchicum seeds to be macerated in 770 of alcohol; this preparation being always uniform in strength, and more active than the ordinary colchicum-wine. Although the above drops succeed so well in acute rheumatic affections, they are of little or no use in old and chronic cases. This induced the author to try the effect of adding minute doses of corrosive sublimate; and although his trials of this modification have been as yet too few to admit of an opinion being pronounced in respect to chronic cases, he has found the addition of great advantage in many cases of the acute form. Although in the various forms of rheumatism in which he has employed this treatment he has not had to have recourse to preliminary bleeding, he by no means denies that this may not be occasionally desirable in the robust.

In treating *acute articular rheumatism* in this way its course has usually been cut short in from the third to the fifth day, convalescence rapidly following, and no trace of heart affection persisting. When the pains have been very severe, tepid applications of a very weak solution of corrosive sublimate have been made to the joints, with the most satisfactory results. Sometimes after a rapid amelioration by means of the colchicum, when the pulse still continues irritable, and the tongue remains loaded, an emetic or purgative expedites convalescence. Among the rheumatic affections of the *mucous membranes*, which may be rapidly and durably cured by means of the opiated colchicum, without the sublimate, may be specified angina, pulmonary catarrh, and influenza, gastric fever, catarrhal diarrhœa, and catarrho-rheumatic conjunctivitis. In the case of catarrhal ophthalmia, even of a severe character, its remarkable efficacy may be watched step by step. Among affections of the *serous membranes*, pleurisy and perihepatitis stand prominently forward as amenable to this treatment. Of parenchymatous inflammations, *pneumonia* has been the only one in which the medicine has been tried, and that only in two slight cases, which recovered with rapidity. In *muscular rheumatism* of the head, loins, &c., from two to four doses have always sufficed. It is also of great efficacy in cases of *rheumatic neuralgia*, especially in facial or intercostal, in sciatica and odontalgia. But the case must be recent, or it will be of no avail. In *odontalgia* the results are truly remarkable, a single dose rapidly dissipating the pain. The distinction between the rheumatic form of odontalgia, and that which arises from carious teeth is exhibited by the different effects of the colchicum. For the relief of odontalgia arising from carious teeth, the author, after having cleaned out the cavity of the tooth, introduces into it a morsel of nitrate of silver as large as a pin's head. In about a minute the moisture of the mouth dissolves this, and the mouth is then to be gargled with cold water, and the pain disappears. He has employed this plan of relieving the pain of carious teeth for the last twenty years, and he has seldom known it fail, even after the ineffectual trial of various other measures. It causes no pain, and it retards the progress of the caries.—*Bulletin de Thérap.—Med. Times and Gazette, April* 14, 1860, *p.* 376.

9.—ON EXPERIMENTAL PATHOLOGY.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Academy of Sciences.

[In a previous lecture (this being an abstract of one of a course delivered at the College of France) M. Bernard showed how necessary a previous study of other sciences was, to a successful study of the complex science of medicine, and that morbid phenomena are merely modifications of those phenomena occurring in health.]

We shall now enter on the study of the symptoms peculiar to the pathological state, the agents which give rise to them, and those calculated to bring about their removal; lastly, we shall produce all the phenomena of disease by artificial means, and shall then endeavour to make them disappear.

But, before entering on this subject, it is indispensable to ascertain whether everything which occurs in a state of disease can be explained on physiological grounds; or, whether disease has the peculiar property of originating, in the living being, laws altogether new, and of which we have not the slightest idea as existing in a state of health.

If, in the case of an adult in the full enjoyment of all his faculties, we ask ourselves what is the regulating agent, what the *primum mobile* of all physiological actions, we are constrained to reply that its seat is in the nervous system. It is to the nervous system that we owe both sensibility and voluntary motion, that two-fold source of all our relations with the external world; its presides over all organic functions, and it is my intention to prove to you, that while it is the origin of all the normal phenomena of life, it is also that of all pathological action.

In proportion as we ascend in the scale of animal life, we see the nervous system acquire greater development, and at the same time we observe that diseases become more frequent, more variable in their form, and more complicated in their nature. Why should this coincidence astonish us? Are not all our organs dependent directly on the nervous system? If we take, one by one, the different systems of the animal economy, it will be easy to show that all the symptoms of the diseases to which they are liable, may be produced by direct irritation of their corresponding nerves. We can even give rise, in this way, to all the anatomical lesions by which they are characterized.

What, for instance, are the principal signs of the affections of the respiratory organs? Cough, dyspnœa, increased bronchial secretion;

are not these the symptoms which most frequently proclaim their existence? Now all these phenomena can be produced at will by the direct excitation of the pneumogastric or certain other nerves; we can even call into existence the anatomical lesions incidental to pleurisy and pericarditis. The causes of these morbid changes would therefore appear to be intimately connected with the nervous system. If we now turn our attention to the digestive apparatus, we shall soon be convinced that the physiologist possesses the same power relative to it, as he does in the case of the respiratory organs. By exciting the solar plexus and its efferent branches, we can determine both diarrhœa and dysentery, together with the anatomical lesions which habitually accompany them. Acute peritonitis has even been induced with all its consequences, as evidenced on opening the animal, by the presence of pus and false membrane in the peritoneal cavity.

Thus, then, a multitude of diseases may be brought into existence by a simple modification of the elements which the animal economy originally contains, without having recourse to the introduction of any new principle; and if we were to examine the other systems of the body, results analogous in their nature would be obtained. Fever itself, that essentially Medical symptom, can be excited by a mere mechanical irritation of the nervous system, and the products of inflammation, such as pus, false membranes, and plastic exudations, may, any, or all of them, be called into existence, in a similar way. In an animal, previously enfeebled, we can produce directly pleuritis with purulent deposit, by the simple division of the great sympathetic nerve; in order, however, to ensure success in this experiment, it is absolutely necessary that the state or condition of the animal's health be previously lowered.

It is, therefore, a fact, that the perverted state of the nervous system gives rise to a great variety of diseases, not only of a general, but also of a local character: deprive a muscle or a bone of its nervous supply, and you will have, as a consequence, fatty degeneration in the one case, and rickets in the other; in fact, if you tie the nerves, which enter the nutritive foramina of a bone, you will very soon see the cells of the lamellar structure increase in size, the vessels become more numerous, and all the phenomena of rickets follow in rapid succession: we can even bring about these results on *part* of a bone, without interfering with the remainder. This experiment has been successfully carried out, in the case of the lower jaw, by M. Schiff of Berne.

But there exists in disease an immense number of other phenomena which, at first sight, it appears impossible to produce by a simple lesion of the nervous system; I allude especially to the alteration or modification of the fluids of the body, which takes place in the course of certain maladies. Now I am prepared to demonstrate that a vast number, if not all, of these morbid changes, are still to be traced to the action of the nervous system, and that they can be reproduced at pleasure by the physiologist. Among the various fluids of the body, the urine is that one the morbid changes of which have been the most carefully and completely investigated. Now you are perfectly aware, that albuminuria, polyuria, and diabetes, are invariably produced by excitation of definite points of the medulla oblongata, the peculiar form of the perverted urinary secretion being determined by the particular portion which is acted on: it is in the case of diabetes especially that the importance of this experimental fact is fully brought out. It was supposed that in diabetic patients the morbid state created entirely new conditions in the economy, which gave rise to the pathological production called sugar; it is now, however, admitted on all hands that these phenomena are fully explained by the mere exaggeration of a normal function, in virtue of which glycose is generated in every individual even in a state of health. It is therefore evident that disease, in this case, is nothing more nor less than an exaggerated natural function.

There exists, however, a certain number of pathological products and morbid manifestations which we have not yet been able to imitate by the employment of artificial means. Shall we be able, at a later period, to connect these facts with those which already fall within the range of experimental physiology? Such is the scientific problem of the day. The question is, whether we shall, one day, be able to embrace pathology in its entire extent, within the compass of biological explanations, or whether we shall, in addition to all which we can imitate or explain, for ever be compelled to recognise a *special principle,* mysterious in its nature, which we must acknowledge as a morbid or vital phenomenon?

Let us take, for example, eruptive fevers, small-pox, scarlatina, and measles; these are diseases, indeed, which it is impossible for us to produce without having recourse to a special virus. Shall we, one day, be able to realize this undertaking without the intervention of the peculiar animal poison on which they seem to depend? Must we not, first of all, solve the question whether such diseases as those we have just specified can possibly exist in animals, even in those which approach most closely to the human species? Are they not in reality the exclusive property of the human organization?

Nothing is more difficult than to produce, through the agency of the nervous system, eruptive diseases in animals, the vitality of the skin of which is essentially different from that of man. We can, nevertheless, produce ecchymosis, congestions, and glandular swellings; but it must always be borne in mind, that each particular species of animal has its own peculiar diseases, which cannot be transmitted to a neighbouring species, however closely allied they may be. Now man, in himself, presents a greater number of special diseases than all the other animals taken together.

Fortunately for physiology, such incompatibilities are rather the exception than the rule. Tubercle, cancer, and many other morbid productions, are found equally in animals and in man. Every disease which gives birth to morbid tissue is evidently a perversion of the nutritive function; now, who will venture to deny the influence which the nervous system exercises over this physiological act? We must, therefore, advance resolutely in the path which lies open before us, without allowing ourselves to be disheartened or intimidated by the difficulties of our science. But we must bear in mind that a disease is not characterized by one single symptom; it consists rather of a complete series of symptoms, standing to each other in the relation of cause and effect. It is, in fact, a morbid evolution which offers a commencement, a middle, and an end; so that a skilful and practised observer, on witnessing the first stage of a disease, can predict its probable termination. This is no doubt true; disease does not consist in an isolated symptom; it is a collection of symptoms. Now these reunions of morbid phenomena, we indubitably succeed in reproducing in animals. The functions of life are modified in various ways by a variety of different agents. Poisons determine real disease which present an unbroken chain of symptoms, consequent on the introduction into the system of the toxic agent. Here, therefore, we find an entire class of diseases which can be produced at will.

But setting aside this question, of such vast dimensions, and to which we shall revert at a later part of our course, let us inquire whether, by mere surgical operations, by mere mechanical lesions, we can determine on the animals subjected to experiment, a certain number of morbid series. If you simultaneously remove the two kidneys of a dog, or simply tie the renal arteries, you immediately produce a general disturbance in the entire economy. The animal is powerless in expelling the excrementitial product which should pass off by this channel, and the whole system becomes gradually poisoned. At first the animal is not seriously affected; it continues to eat and digest its food for a certain lapse of time, which corresponds with the period of incubation in diseases; by-and-by it is attacked with vomiting and purging, shortly after which it dies.

What takes place in a case like this? Let us endeavour to explain it. During the first period the urea, which can no longer be eliminated by the kidneys, is expelled by the intestines. It is found, together with the salts of ammonia, in the animal's excrements, and even in the gastric juice. If this new mode of elimination could be prolonged indefinitely the animal would not become diseased—it would not die; but very soon the mucous membrane of the intestines, irritated by the constant contact with the ammoniacal salts, gives rise to morbid changes. On the other hand, as long as the urea is eliminated by the intestines, it does not find its way into the blood. This fact has been been demonstrated experimentally by MM. Prevost and Dumas, who have not, however, succeeded in explaining it. Now, at a later period, when the mucous lining of the intestine refuses to continue this function, which is altogether foreign to it, the urea finds its way into the blood, and the animal soon expires, comatose and convulsed.

When the cessation of the urinary secretion depends on the ligature of the renal arteries, this state of things may sometimes be obviated by removing the ligatures; the self-same thing would also take place in man, if there existed an obstacle to the passage of the urine, and if it were possible to remove that obstacle; but in all cases in which the kidneys has been removed death has always supervened. The destruction of the animal has been the invariable termination of the morbid series.

Here, then, we have a disease which can be artificially produced; but there are many others, the causes of which are agents existing exterior to the body; contagious affections belong to this class. The fact has been experimentally proved in the case of peri-pneumonia of horned cattle, by the establishment of a communication between two cow-houses, the one containing healthy, the other diseased cattle. In the inclosure, which, in the first instance, contained only healthy cattle, several cases of this peculiar affection occurred in succession. —*Medical Times and Gazette, Jan.* 21, 1860, *p.* 55.

10.—ON DISEASES ARISING FROM THE VITIATED DEVELOPMENT OF CELLS.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

We are compelled to admit the existence of three principal classes of phenomena within the living body, which, although closely connected through reciprocal influence, exist independently of each other.

Firstly, Nervous phenomena, embracing all the mechanical actions of life;

Secondly, Catalytical phenomena, embracing the various kinds of fermentation;

Thirdly, Histological Phenomena, embracing the entire results of cellular evolution, or the process of development.

The two first classes of physiological agents, viewed in connexion with the origin of disease, have been examined in our previous lectures; it now remains for us to investigate the latter,—viz. the morbid phenomena superinduced by disordered powers of evolution.

It cannot be denied that during the first period of embryonic life, the development of tissue is in no way connected with nervous influence. The nerves have not yet been called into existence, and are only created at a later period, through the agency of that mysterious orce which presides over the evolutions of the being so lately calledf into life. The vascular system being, for similar motives, set aside, we discover, in the very first stage of existence, nothing but the primitive cell, and the medium in which it grows to maturity. But micrographists do not entirely agree in the view they adopt of the first step which commences that long series of successive evolutions, by which an entirely new living being is ultimately produced. That within an entirely amorphous medium, a living cell might spontaneously be produced, was the opinion of Schwann; but, in our own days, these views appear to have been completely laid aside; modern investigators are convinced that all living organisations are invariably derived from one primitive cell, which, by its divisions, and subdivisions, gives birth to all the rest; now, this primordial element originates, of course, with the animal's parents.

We meet, however, with cases in which it seems altogether impossible to deny that one or several primitive cells have been spontaneously generated within a medium which previously contained no vestige whatever of histological elements. The serum of blood, for instance, when introduced into a vessel hermetically closed, is found to contain, after a few days have elapsed, a large number of yeast-cells, which the grape-sugar dissolved in this liquid has probably produced; and yet, when carefully examined under the microscope, immediately after coagulation had taken place, it did not contain a single nucleus or cell. Similar results frequently occur for notice; and, in more cases than one, the mere process of subdivision and multiplication will not be found sufficient to account for cellular development.

In the adult the phenomena of evolution continue to exist, and although far less extensive than during fœtal life, they are almost equal in importance. Thus, in certain animals, organs are found, which momentarily disappear, and are subsequently reproduced. Hunter observed that the sparrow's testicle, reduced during winter to the smallest possible bulk, promptly returns to its primitive size during the first days of spring. Mr. Stanlius has collected all the cases of similar reproduction which have been noticed in the animal series; thus in hibernating mammals, for instance, certain parts of the body are completely atrophied during winter, their functions being suspended for a time; some nervous ganglia are even found to disappear, more especially those connected with the genital organs; but as soon as the physiological activity of these parts is awakened, they are called a second time into being. Hunter also discovered that in the pigeon a peculiar organ, no vestige of which exists in the animal's previous state, is produced during the latter part of incubation; the mucous membrane of the gizzard becomes tumefied, grows vascular. and secretes a new substance, which serves for the nourishment of the young birds when newly hatched. In the human species (as in all mammals besides), the rapid development of the mammary glands, towards the period of lactation, offers another instance bearing on the same point.

But must we consider these as the only phenomena of a similar nature which take place within the system? Do not mucous surfaces continually secrete new layers of epithelial cells? and is not the epidermis incessantly renewed on the surface of the skin? Here, then, we discover evident instances of histological development going on during all the stages of life. With respect to muscular tissue, its tendency to increase in bulk under the effects of constantly renewed exertion is perfectly known ; but it has been hitherto admitted that the constituent fibres of a muscle may no doubt increase individually in size, but that their number invariably remains the same. A German micographer, Mr. Budge, has lately endeavoured to prove the contrary. A frog, being reduced by abstinence to a state of emaciation, a small muscle is laid bare, and the number of its elementary fibres ascertained by direct investigation under the microscope. The animal being then properly fed, and gradually restored to a state of health, the same muscle is again examined ; and, according to the ingenious observer, the number of its elementary fibres is found to have considerably increased, as well as their bulk.

Let us now consider the morbid manifestations of this power, which never suspends its action within the living body; we allude, of course, to those peculiar tissues which have been styled heteromorphous—an expression utterly condemned by German micographers; for morbid tissue is generated within the economy in strict conformity with the laws that preside over fœtal development. But as we have previously seen, both nervous influence, and catalytical agency, give rise to a variety of diseases when deviated from their proper course; thus, also, in certain given cases, the power of histological evolution may create positive disorder in the system. An immense and uninterrupted movement takes places within the organs of which the body is composed, for the purpose of supplying new tissues, in the place of those which are no longer fit to accomplish the functions devolving upon them; let this unceasing activity be diverted from its proper channel, and the production of tubercle, cancer, and all kinds of morbid deposits, will be the immediate consequence. We find here as usual, an evident connexion between the phenomena of health and disease, between physiological activity and pathological influence. The question which lies before us must evidently be viewed in this light; and such is the principal object of Virchow's labours on cellular pathology, the leading features of which it is our purpose to nake known to you. But before entering into the study of this particular point, a few general notions on the subject cannot safely be dispensed with.

Some diseases, in the first place, result from total absence or considerable deficiency of normal evolution on a given point. The mucous coat of the intestinal tube affords us a fine example of incessant development. New layers of epithelium are continually being secreted, to line its inner surface ; but a living medium, or blastem, is necessary to their production ; and whenever this blastem itself happens to be altered in its essential properties—a modification which always occurs in inflammation—the epithelium disappears, and is no longer regenerated. Cholera also exhibits another instance of this ; for it has been indisputably proved that, in this disease, the vessels which ramify on the internal surface of the intestines are completely laid bare. In his admirable researches on the intestinal mucous membrane, Professor Goodsir has established that, after each meal, when absorption has taken place, the epithelium which covers the villosities falls off, and is renewed during the interval which elapses before food is again introduced into the digestive apparatus—a remarkable instance of the rapidity with which the reproduction of tissue frequently takes place. But when, through some pathological agency, epithelium is no longer secreted, what results from its absence? No obstacle is henceforth opposed to serious exudation from the vessels; no protecting surface resists the introduction of various poisons into the economy; and, lastly, no regulating power of absorption any longer exists. In this manner innumerable diseases may be traced back to the suspended activity of normal evolution as their primary cause. The chronic inflammation of the trachea and bronchial tubes likewise destroys the vibratile epithelium, the utility of which is too well known to be expatiated on.

But we meet, at the same time, with other diseases, which arise, not from interrupted, but from perverted evolution. Cells which pursue a regular course in their development comprise three distinct elements—Firstly, an envelope, or cellular paries, the physical properties of which take a prominent share in its action; secondly, liquid contents, the importance of which is principally derived from their chemical composition; and, lastly, a nucleus, in which the powers of development appear to reside. As soon as a morbid state of nutrition supervenes, the contents of the cell are liable to alteration. Whether pigment, or fatty substances, or carcareous salts are therein deposited, morbid tissues are gradually formed, and disease is introduced into the system; and, even in similar cases, no pathological entity, no abstract principle of disease, is required to explain the fact. The deviation of physiological activity is its only cause. It is therefore evident that, in their successive phases of development, heteromorphous tissues entirely resemble the normal ones, and are subject to the same natural laws. To Müller belongs the honour of having been the first to proclaim this great principle; and he may therefore be deservedly styled the creator of cellular pathology. He was the first to open that path in which Virchow now treads with so much success.

The intercelluar tissue, or blastem, is the medium from which the cells derive the elements of their formation; it is, according to Virchow's picturesque expression, their territory. Now there exist various conditions in which the blastem no longer contains the principles required for the normal developement of cells; it is, for instance, indispensable that it should always contain glycose, albumen, and fat; the absence of a single one of these three substances is an insuperable barrier to cellular evolution; and we, therefore, constantly find them existing as well in the tissues of the embryo, as in those of the adult. But a variety of other conditions, essentially injurious to histological growth, may casually arise; and the existence of morbid blastems, which give birth to all tissues endowed with abnormal properties, may easily be conceived as of possible occurrence. Such are, no doubt, those very general dispositions of the economy, known under the name of diatheses, and which, when once they have firmly established their hold on a previously sound individual, are capable of being transmitted to his posterity; we must evidently consider them in the light of conditions of existence entirely new, which in the first instance are accidentally produced (for disease must evidently begin somewhere), but which when once called into existence, exhibit a strong tendency to maintain themselves in being. Thus, when food, insufficient in quantity, or of an unwholesome kind, has ultimately reduced to a consumptive state an animal previously enjoying perfectly sound health, its offspring often inherits the morbid disposition which, in the parent, was entirely accidental; and syphilis, that well-known and fruitful source of heteromorphous productions, is similarly transmitted from parent to child.

Such pathological dispositions, or diatheses, result from causes various in their nature, but which concur in one point, viz. the disposition which opposes all modifications favourable to the patient's health. Sometimes they are the result of a profound change in the fluids of the economy; sometimes they originate in the introduction of peculiar poisons, which, after having once penetrated into the system, can in no way be expelled: if there existed, for instance, a poison which none of our organs could eliminate, it is clear that after penetrating into the torrent of the circulation, it would nowhere find an issue, and would in consequence become the origin of permanent modifications in the economy. The possibility of a similar case may be rationally conceived, by referring to the singular fact already mentioned, that iodine when once introduced into the blood, is not eliminated before a long space of time, on account of the affinity which the salivary glands exhibit for this substance; we have, therefore, in this case an instance of a body which cannot (for a time at least) be expelled from the system; the animal is, therefore, during that period, laid under an iodic diathesis.

Viewing the subject in an entirely physiological light, it may be contended that individuals affected with local cancers are not properly, so to speak, in a state of disease, as long as the organs affected are not altogether essential to life; but when cancer attacks the limbs, the possibility of a Surgical cure may at least be presumed, if not expected actually to take place; and the patient is not really diseased—that is to say, life is not directly brought into danger. Thus, when cancer attacks the liver, if the disease is not too extensive, the morbid productions are separated by large tracts of sound tissue, which fulfil, as in the healthy state, their physiological duties; bile is secreted as usual, and grape-sugar exists within the glandular tissue. But when, at a later period, the disintegration of the elements which constitute the morbid production have poisoned in some measure the whole economy, by pouring into the torrent of the circulation fluids impregnated with the noxious principle, then, indeed. the affection becomes a general complaint, and its nature entirely changes. Cancer is not a diathesis in itself; but the subsequent cachectic state is evidently diathetical.

To conclude the history of these morbid evolutions, there yet remains one to be described; and this is imperfect nutrition. It is evident at the present day that the anatomical conditions, brought so prominently forward in Bichat's celebrated work, are quiet insufficient to explain all the various modes of dying. Experience has taught us that patients often die without offering, in the post-mortem examination, the slightest modification in the anatomical condition of their organs. In the course of our physiological experiments we often see dogs arrived at the very last stage of emaciation, although the appetite continues unimpaired till the last moment. They sink from sheer exhaustion, while the lacteals are gorged with chyle; and, when opened, their bodies offer no trace whatever of pathological alterations.

The latent cause of this singular process is, that nutrition, when considered within the depths of our organs, is, in fact, nothing more than a peculiar mode of evolution. The economy produces within itself substances indispensable to life; glycogenous matter affords us an example of this: formed within the body by a special process, it plays an immense part in histological phenomena. As soon as it fails to be supplied. epithelium is no longer produced; various diseases are the immediate result; and, under similar circumstances, life is inevitably brought to a close. The physiological act called nutrition, comprehends, therefore, two distinct parts: formation of cells is the first; creation of blastems is the second; and the latter is no less indispensable to our existence than is the former; as soon as pathological influences arrest either the one or the other, death is the consequence. There exist, therefore, two distinct modes of dying: sometimes life is cut short at once by an important injury to some essential organ; sometimes, on the contrary, it gradually fails through imperfect nutrition; and this latter termination is the ordinary result of acute diseases, when they prove fatal. In certain cases, for instance, glycogenous matter is no longer produced; and after a given space of time the patient dies, although the appetite remains unimpaired till the last moment. In making the autopsy, the lacteals will be found in a state of repletion; but when analysed, the fluids of the economy no longer present the slightest vestige of sugar. Death then supervenes, and is the mere result of suspended activity in organs for which proper nourishment is no longer provided.

You therefore see, that to create laws especially intended for the use of pathology cannot in any case be justified; and that physiology furnishes, in every possible condition of health or disease, a key to the interpretation of vital phenomena. These general notions I look upon as indispensable to the study of particular points: it now remains for us, in order to complete this general survey, to examine the all-important question, "Whether medicines act on a sick patient in the same manner as on a sound individual?" and how far the results obtained in one case are fit to be compared with those observed in the other. It is our intention to examine this subject: its study is an indispensable introduction to the various investigations we are about to undertake; for, after producing artificially—no matter how—a morbid state in an animal, we shall have recourse to the counter-proof, by seeking for therapeutical agents to effect its cure.—*Med. Times and Gazette, March* 3, 1860, *p.* 209.

11.—*On the Study of Disease by Synthesis.*—At the Medical Society of London, Dr. Richardson read a paper, the third of the kind he has written on the subject, called the "Study of Disease by Synthesis." In detail, as Dr. Richardson admits, diseases have been studied by synthesis for ages; since the unknown time, in fact, when inoculation for small-pox was first performed. But it is his design to reduce this study by synthesis, or, in other words, the study of disease by inducing diseases artificially, to a perfect system adapted to the entire investigation of disease by philosophical experiment. Towards this end, Dr. Richardson divides diseases for study into six great classes—viz.: parasitic diseases; zymotic diseases; diseases the primary origin of which is in the nervous system; diseases in which from a malchemistry a new growth is evolved from the economy, and supplied out of the organic materials—malignant diseases; diseases due to simple transformations of natural tissues—degenerations; and, inflammations. In the paper to which we refer, the most interesting point adduced related to the question, whether inflammation is or is not a local change. According to Dr. Richardson's view, derived from his experiments in the production of endocarditis, there is no possibility of local inflammation unless there be brought to the part to be inflamed a foreign agent which shall excite a primary local change—congestion. But this excitation set up, all else—all future products, even to pus—are purely local, and may be independent of the condition of the blood. There is thus, according to this argument, no abstract difference between inflammation of the skin caused by a blister and rheumatic endocarditis. Such difference as exists is, in short, merely in conveyance of the exciting body: in the case of the blister, the irritant is conveyed by the hand to the part—in endocarditis, by the blood. The results are the same.—*Med. Times and Gazette, Feb.* 4, 1860, *p.* 121.

DISEASES OF THE NERVOUS SYSTEM.

12.—THREE CASES OF TETANUS, IN WHICH WOORARA WAS USED IN THE TREATMENT.

By T. Spencer Wells, Esq., F.R.C.S., Surgeon to the Samaritan Hospital, &c.

[So early as 1811, Sir B. Brodie experimented on the action of this poison, and more recently Professor Sewell at the Veterinary College, and Mr. Morgan and Dr. Harley (an article by whom appears in our last volume, p. 37). On the Continent, Vella, Manec, Chassaignac, and Bernard have recently experimented upon the application of woorara in the treatment of tetanus in the human subject.]

The author detailed the particulars of a case of chronic but severe trismus, going on to opisthotonos, appearing a fortnight after ovariotomy in a patient forty-one years of age. The exciting cause appeared to have been a draught of cold air. The great point of interest in the treatment was, that woorara was used hypodermically and epidermically, six grains of the extract having been used in six days. The patient recovered.

In a second case, acute tetanus appeared seven days after ovariotomy in a lady thirty-eight years of age, and proved fatal in three days. The treatment was commenced by assafœtida injections, and woorara was afterwards used, the softened extract having been inoculated in both arms.

In the third case, tetanus appeared four days after a simplified perineal operation for the relief of prolapsus uteri in a patient fifty-one years of age. It progressed slowly; was treated first by opium and ether, afterwards by woorara, and latterly by chloroform, the influence of which was kept up at intervals for forty-eight hours. The patient died on the evening the paper was read.

The author, after expressing regret for the little positive information he had been able to lay before the Society as to the real value of woorara in the treatment of tetanus, thought that he had seen enough to establish the following propositions:—

1. That our knowledge of the physiological action of woorara—of its antagonistic effects to the artificial tetanus of strychnine; of the results of its use in idiopathic tetanus of the horse and ass; and the facts that two cases of chronic tetanus in man on the Continent, and one in this country, have recovered during its application—should encourage further experiments.

2. That although three cases of acute traumatic tetanus in man on the Continent and two in this country have died, notwithstanding the use of woorara, this should not discourage us from further trials, when we consider the very fatal nature of this form of tetanus, and the fact

that in only one of these cases was the woorara applied in a large quantity.

3. That looking to the probable difference in strength of the specimens of extract brought to this country, and to the well-founded belief that they are not all prepared from the same species of vegetable it would be well, in future trials, to use a solution of the active principle of woorara—the alkaloid curarina.

4. That it is desirable to ascertain, as far as possible, by experiments on the lower animals, what dose of this alkaloid might be inoculated with safety in man; and whether the artificial tetanus of strychnine establishes a tolerance of curarina. This would afford some test as to the safety of using much larger doses in man when suffering from tetanus than when in a state of health.

5. That veterinary surgeons should be requested to aid us in our attempts to determine the value of woorara when treating tetanus in the lower animals, by carrying on the dose of the poison until its full effects were shown by suspended animation, and then restoring the animal by artificial respiration.

Dr. HARLEY, after complimenting Mr. S. Wells on the able manner in which he had treated his subject, said that he agreed with the author in believing that the conflicting opinions regarding the action of woorara on the human body were chiefly due to the fact of the specimens employed not being of uniform strength. He had in his possession at the present time five specimens of the poison prepared by different tribes, and, although the general action of all was identical, yet their strength varied considerably. There was, however, a second and equally important reason—namely, the mode of administration. Woorara differed from many remedies in not being absorbed equally and readily when introduced into the body by various channels. Thus, it could not enter through the unabraded skin, and when taken by the mouth, its action was extremely doubtful. Dr. Harley had made a pigeon swallow twenty and a mouse thirty times more than was sufficient to destroy them if introduced by a wound, and yet both remained unaffected. This did not arise, he said, from the poison being destroyed in the digestive canal; for he had seen Bernard poison a bird with the excrements of a dog, to which a poisonous dose of woorara had been given with impunity. As regards the value of woorara when compared with other narcotics, Dr. Harley remarked that he considered that its superiority consisted in its peculiar power of paralysing the motory, and not the sensory nerves—nay more, he said that it could be so administered as to destroy entirely the power of voluntary motion without impairing the consciousness of the animal. (Dr. Harley related one or two experiments in proof of this statement.) In administering woorara to animals labouring under tetanus induced by strychnine, he therefore gave sufficient to paralyse all the muscles except those of respiration. In this way he was able to allay the tetanic spasm without destroying the intelligence or arresting the performance of the organic functions; and by continuing the thus moderated action of woorara until the kidneys had time to eliminate the strychnine from the system, he had been able to save the life of the animal. The theory of its action in tetanus was, Dr. Harley considered, much the same—that is to say, you try to keep the spasms from killing the patient by their violence until the morbid state calling them into play has exhausted itself. The chances of curing by woorara a patient labouring under strychnine-poison, are naturally much greater than those of curing a case of traumatic tetanus, where the morbific matter may go on increasing until its effects are more than sufficient to counteract the influence of woorara.

Dr. Sibson said, that as far back as 1838, he had been in communication with Mr. Waterton, on the subject of woorara, and that gentleman, after hearing of Mr. Sewell's experiments, went over to New Guinea to obtain a quantity of the poison, with a view to its employment in cases in tetanus and hydrophobia. He subsequently made experiments upon an ass to prove that an animal might be resuscitated, after what would have otherwise been a fatal dose, by the employment of artificial respiration. The experiments to produce the artificial respiration were performed by himself (Dr. Sibson). The animal lay apparently dead for upwards of an hour, the beating of the heart being the only sign of life which it exhibited. It was, however, at length made to breathe, and an hour afterwards Mr. Waterton walked the animal round the room. He (Dr. Sibson) subsequently experimented with some of the poison on a horse affected with tetanus. He administered a dose proportionate to that given to the ass, but no decided effect was produced, and on increasing the dose the animal succumbed. In another instance the horse respired at the end of three or four hours, but before the apparatus could be applied a second time, it died. During the whole time the experiment was going on, the animal was free from tetanus. He had also experimented on the lower animals in a manner similar to that described by Dr. Harley. No considerable result was obtained except in one instance, and in this the poison was administered by the mouth. He (Dr. Sibson) considered, however, that in this case some accidental abrasion had occurred. In his opinion tetanus could only be overcome by giving such a dose as would absolutely require the employment of artificial respiration. For years past he had been prepared to sustain the responsibility of employing artificial respiration in cases of hydrophobia; but hitherto he had had no opportunity of testing its effects. The woorara poison he considered offered the best chance of success, from its complete destruction of the symptoms of the disease, for some hours at least. He therefore trusted that it would yet be tried, combined with artificial respiration, in some cases of hydrophobia. He was apprehensive, however, that tetanus would be found to be too severe a malady to be overcome, without, indeed, the means resorted to should destroy the patient.—*Lancet, Dec.* 3, 1859, *p.* 562.

13.—*A few Particulars respecting Woorara.*—M. **Bonvier**, relying upon a work published by M. Reynoso, gave, at a late meeting of the Surgical Society of Paris, the following particulars respecting the composition of woorara:

There is a true and false woorara; the two are very different, but extremely difficult to distinguish from each other. The true woorara presents, moreover, several species. This substance is, therefore, not always obtained of the same strength; it comes from different countries, and is extracted from one or several plants, which contain one identical principle, the character of which is to cause death when injected into the blood, and to be innocuous when taken into the stomach.

There is, however, one sort of woorara which acts on the gastric mucous membrane of certain animals, at given ages; which circumstance would tend to show that much difference exists in some samples of woorara.

It is well known that it is not always prepared from the same plants, nor from plants of the same nature. It has even been shown by M. Chombrook, that one kind of woorara is obtained from a great number of plants, almost as great as the number of ingredients entering into diascordium or theriacum.

Amongst the plants used are some strychneæ, but the rest has not as yet been determined botanically. It is doubtful whether snake poison is mixed with it.

Gunelli was the first who, in 1758, insisted upon the innocuous nature of woorara when taken into the stomach. Lacondamine and Humboldt corroborated his statements. Man can eat with impunity animals killed by woorara. Death by the poison occurs generally by paralysis of the motor nerves.

It is of importance to try the woorara in various manners before administering it to a patient, and to ascertain whether it produces no poisonous effects when introduced into the stomach, and also whether it paralyses motor nerves without affecting the nerves of sensibility. These precautions are indispensable, for there is a kind of woorara which may kill by gastric absorption.

As to the occasional inefficacy of the poison when inoculated, the experiment of M. Dequise may be mentioned. This surgeon had been given, by a traveller, a quiver full of arrows said to be poisoned with woorara. He found, however, on trying them upon a dog, that they produced no effect. If we consider the woorara as an extract, such a result need create no surprise, as extracts are very liable to change.—*Lancet, Dec* 3, 1859, *p.* 567.

14.—ON TETANUS.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

[The following clinical remarks were made by Mr. Hilton on a case of tetanus, which lately died in Hospital.]

This disease may kill by spasmodic action of the muscles of the larynx, by spasm of the heart, or by general exhaustion of the nervous power. I once opened the larynx between the cricoid and thyroid cartilages, in a case of tetanus, in which immediate death seemed to be threatened by the closure of the larynx. The patient breathed freely through a tube introduced into the larynx, and the whole of the laryngeal distress ceased. The general spasm was much subdued during several hours, and then returned in severity, and he died suddenly, during a paroxysm of spasm affecting the heart, as proved by the post-mortem; for the heart was found firmly contracted, and nearly free from blood. Only a very small quantity was found in the right side. The mode of death in this case was spasm of the heart. The patient, whose case forms the basis of this lecture, died from exhaustion, or gradual subsidence of nervous power.

It is reported in the case before us that trismus, or lock-jaw, was an early symptom. It is not at all necessarily the first indication of tetanus. Although the disease is called "locked jaw," it often happens that twitchings of the muscles in the trunk or extremities manifest themselves as symptoms of tetanus before locked jaw; still it is an early and very prominent and almost constant symptom. What is the explanation of this local symptom showing itself usually so early? Experiment indicates that the grey matter of the interior of the spinal marrow is probably the structural seat of tetanus. The fifth nerve, or nerve of mastication—the one involved, and which must be the direct cause of trismus—has a larger connexion or continuity with the grey matter of the spinal marrow than any other nerve in the human subject; and in this fact, perhaps, lies the explanation of the early symptom of locked jaw; and no doubt the firm closure of the lower upon the upper jaw depends on the relative greater strength of the muscles closing the mouth, as compared with those which depress the jaw. It is curious to observe the gradual ascent of the cause of tetanus—to see how the disease encroaches upon the higher or anterior nerves of the base of the brain, ultimately reaching the third cerebral nerve. Then the muscles which are supplied by this nerve becomes tetanic, and cause retraction of the globe deep into the bony orbits. * I have repeatedly observed this latter symptom in tetanus; and it is, I believe, always a very serious indication; for it points to the great extent of the structural lesion in tetanus, although what may be the exact character of the pathological state is not explained.—*Med. Times and Gazette, March* 3, 1860, *p.* 219.

* Since delivering this lecture I saw, with Mr. Mavor, of Park Street, a horse suffering from tetanus; and he observed that as the tetanus goes on, we shall almost lose sight of the eyes, they will become so deeply buried in the orbits.

15.—*Case of Traumatic Tetanus treated by Internal Use of Chloroform.* By Dr. A. Dick. Buenos Ayres.—[The patient was a mulatto sailor, aged 32. Admitted into British Hospital, Buenos Ayres, under Dr. Dick, Dec. 27, 1858, with two slight incised wounds, one in the scalp, the other in the hand, inflicted five days before. On Dec. 31, premonitory symptoms of tetanus appeared, which on the following day were more marked.]

Jan. 2. All symptoms aggravated, and, in addition, frequent spasmodic attacks, amounting to opisthotonos. During Dr. Dick's visit, had three of these paroxysms, the least touch or breath of air serving to produce them; profuse perspirations. Ten drops of chloroform every twenty minutes. The patient being minus a tooth, it was administered through hiatus. Shortly after administration of chloroform the paroxysms ceased, but the teneness and rigidity of cervical and abdominal muscles continued.

Jan. 3. Pressure on abdomen did not produce spasms, which had previously been the case, and observed by Dr. Dick in all cases he has seen. Chloroform, gtt. xxx. every half hour; olei croton. tig. gtt. iv., his bowels not having been moved since 30th.

Jan. 4. No paroxysms; muscles still hard and unyielding; quantity of dark fetid matter passed from bowels. Chloroform, gtt. xxx. as before.

Jan. 5. Pulse less rapid; no paroxysms. Continue as before.

Jan. 6. Id., id. Croton oil repeated. The same treatment continued up to night of 12th, *when the attendants having neglected to give chloroform, he had a very severe attack of eclampsia.* Neglect being guarded against, he had no return.

Jan. 14. Has slept for first time since invasion of disease, and bowels begin to act naturally.

From this time the symptoms began to mitigate. He had less difficulty in swallowing; the cervical and masticatory muscles became slightly moveable.

Jan. 23. Pulse very weak. Expresses desire for animal food. Chloroform, gtt. xxx. every two hours, to be discontinued after midnight.

Jan. 30. Muscular movements free and almost natural, save occasional convulsive twitch in wounded arm; speech not yet quite natural.

Feb. 10. Still in hospital; weak, but otherwise in good health.—*Edin. Med. Journal, Jan.* 1860, *p.* 640.

16.—ON EPILEPSY.

By Isaac Pidduck, M.D., London.

[Dr. Pidduck remarks that morbid anatomy failing us in our study of this disease, we are driven to a careful study of its symptoms, in order to arrive at a successful mode of treatment. The subjects of idiopathic epilepsy are for the most part remarkable "for the large size and high temperature of the head".]

The symptoms of epilepsy are those of a sudden explosion of accumulated nervous energy. From the periodical recurrence of the fits in many cases, it is inferred that the accumulation of nervous energy (*dynamis neurotica*) goes on for a definite time in the brain and spinal cord, till at length an explosion ensues upon the muscles of voluntary motion, which are thrown into violent action, and by this means the accumulation is exhausted. This explosion is followed by languor, and frequently by a state of coma, or prolonged sleep—indubitable signs of exhausted nervous energy.

There are two modes of preventing this gradual accumulation and sudden explosion of nervous energy, constituting the fits of epilepsy—namely, the natural and the artificial. The artificial is by the use of those medicinal agents which keep up a constant involuntary action of the voluntary muscles, such as strychnia internally, and electro-galvanism externally. The natural mode is by the constant exercise of the voluntary muscles. By these means the recurrence of epileptic fits may be deferred almost for an indefinite period—the next best thing to a cure. To the artificial mode there must, of course, be a limit; but not to the natural. By cold affusion over the head daily, to diminish its heat, and by the administration of strychnia, so as to produce a decided twitching of the muscles, the fits, of monthly, weekly, daily, and hourly recurrence, have been deferred for several months, and the same favourable result has been obtained by electro-galvanism.

But the most permanent benefit has been obtained by the natural method, which is the safest and best. In proof of the accuracy of this theory and the success of this practice, the cases of three of the greatest generals that ever lived might be adduced—Julius Cæsar, Napoleon, and the Duke of Wellington. It was in the later periods of their lives, after their active career was ended, that these giants of nervous energy became the subjects of epilepsy. In the case of our own great Duke, the recurrence of the fits was prevented by his temperance in living, by the activity and simplicity of his habits, by the multiplicity of his avocations, and by the hardihood of his amusements and pursuits.

The example of the Duke of Wellington is proposed for the imitation of epileptics who are desirous of enjoying immunity from the fits, in preference to all artificial modes of cure. The confinement of insane epileptics in asylums, though in many cases necessary for the sake of others, is highly prejudicial to the patients themselves, (unless accompanied by hard labour,) as leading to a life of inactivity, which favours the recurrence of the fits. That the cerebral disease found in some cases after death is the effect, and not the cause, of the paroxysms, there is little doubt. It is upon the principle of rousing the dormant nervous energy, and promoting its expenditure, that some of the remedies said to be successful in the cure of epilepsy have operated, such as drinking the warm blood of a slain gladiator,—a miserable remedy, only rendered tolerable by a more miserable disease,—being stroked by the hand of a criminal immediately after execution, swallowing live spiders, &c.

Upon the same principle, paroxysms of hysteria, which, in some cases, so closely resemble those of epilepsy as with difficulty to be diagnosed, are frequently prevented—namely, by making an impression upon the nervous system through the medium of the mind or body, by which the patient is roused into activity and consequent expenditure of the nervous energy.

As strychnia is advised for a remedy in epilepsy, it may be a safeguard against its poisonous operation in an overdose, to mention that camphor is the appropriate antidote to this poison. This fact was communicated by the writer in an article published in the 'Lancet.' He is induced to mention it again in consequence of a case of poisoning by nux vomica recorded in that journal for Oct. 22nd, commencing with this statement: "Nux vomica is one of those poisonous substances for which we unfortunately possess no antidote;" and, again, speaking of poisoning by strychnia,—"There is no remedy for it." Camphor, in five-grain doses, dissolved in mucilage, puts a stop to the tetanic spasms, and gives time for the action of the stomach-pump or emetics, if there is reason to believe that any of the poison is retained. —*Lancet, Dec.* 31, 1859, *p.* 663.

17.—PARALYSIS AGITANS REMOVED BY THE CONTINUOUS GALVANIC CURRENT.

By J. Russell Reynolds, M.D., Assistant Physician to the Westminster Hospital.

[The patient was a married man and father of twelve children, aged fifty-seven. A carpenter by trade. For the last five years he has had much anxiety with regard to his children, though it is not certain that this is the cause of his malady. Occasional tremor of the right arm and leg appeared first, about two years since, and seemed to be increased by anything requiring expenditure of nervous power. These symptoms have much increased lately. The arm is worse than the leg; and in it the motion most constant and most extensive, is that at the shoulder joint.]

The involuntary movement of the arm can be arrested by his lying on the sofa, and pressing the forearm against the ilium; but any attempt to move the limb voluntarily at once reproduces the shaking, although he remains in the recumbent posture. The movement is, moreover, instantly arrested by my firmly grasping either the forearm in any part of its upper two-thirds, or the arm in its lower third. This is not a mere mechanical arrest of the movements, for it cannot be effected by holding the wrist; and the jerking recommences, if, while the extremity is grasped in the manner described, the patient makes any attempt at a voluntary movement. The pressure is not painful, nor is it so directed as to arrest the circulation.

The mental condition of the patient, and his general health, appear unaffected.

Sensibility is unchanged in the right upper extremity; there is no deviation of the tongue, nor distortion of the features. He can walk well, and without dragging either leg; there is occasionally tremor of the right leg.

A continuous galvanic current (direct) was applied to the arm and forearm, the movements of the latter being at the time arrested by pressure. At the end of five minutes he could execute voluntary movements without the least tremor, and emotional excitement failed to reproduce the jerking. The temperature of the two arms, examined after the current had been passing for half an hour, was equal. The involuntary movements did not return until three hours after the current was discontinued; they then reappeared, and continued throughout the evening; stopped at night, but returned on the following morning.

October 6th. The current was applied while the arm was in violent movement, but in two minutes it became perfectly still. Application continued for an hour.

7th. Last evening there was no jerking nor tremor for five hours after the current was discontinued; then it commenced, but stopped spontaneously in about half an hour, and during the remainder of the evening there was nothing more than very trifling tremor. The jerking has returned this morning, but is much less than on the first day of observation. There are but twenty alternations in fifteen seconds, and the range of movement is from three to four inches. The movement therefore, is only ·86 foot per second—less than one-eighth of what it was three days ago.

The current was applied on the 7th, on the 8th, and 10th, and after the 10th—*i.e.*, after five applications—the spontaneous jactitation completely ceased. When any weight is held in the hand, and it is lifted towards the mouth, there is tremor; but this is slight, is not more than has occurred for the last two years, and it immediately ceases when the effort is discontinued. The arm and hand are weak; every movement can be executed by them voluntarily, but such movements are feeble.

28th. Has written me a letter in a good and legible hand.

The current was applied about every other day, for an hour, until Nov. 10th, and during this time there was steady increase in the power of the limb, and the jactitation did not return. No medicine of any kind was given.

November 12th. Quinine and iron were ordered.

15th. W. F—— is in perfect general health; there is no jactitation, and only the slight tremor already described when the hand, with something in it, is raised towards the mouth.

The current employed in this case was derived from Pulvermacher's chain battery of 120 links.—*Lancet, Dec.* 3, 1859, *p.* 558.

18.—*On Electricity as a Means of Diagnosis in Cases of Paralysis.*—[The following extract from a work lately published by Dr. ALTHAUS on 'Medical Electricity,' gives shortly and clearly our knowledge at the present time on the vexed question of electricity as a means of diagnosis in cases of paralysis.]

We have thus arrived at the result that the muscles of paralysed limbs may present three different conditions when subjected to the action of the electric current, and that this may enable us, in certain cases, to form the diagnosis of the paralysing lesion.

1. If the excitability of the muscles—or rather the polarity of the motor nerves—be *increased* in the paralysed limb, the case is one of *cerebral paralysis*, connected with an irritative lesion within the cranium.

2. If the excitability of the muscles be nearly or totally *lost*, we have, in all probability, either *lead palsy* or *traumatic paralysis;* but it must be kept in mind that certain hysterical and rheumatic palsies of long standing present the same peculiarity; and that it may also be found in cases of disease of the brain and cord.

3. *If paralysed muscles respond readily to the electric current*, there is no lead in the system, nor is the connection between the motor nerves of the paralysed muscles and the cord interrupted; but if such cases are of *long standing*, they are due to *brain disease;* and if they are of *recent standing*, they are generally instances of *hysterical, rheumatic*, or *spontaneous* paralysis.—*Dublin Quar. Journal, Nov.* 1859, *p.* 424.

19.—ON THE IMPORTANCE OF FREE RESPIRATION IN CERTAIN STATES OF THE BRAIN.

By W. C. HUNTER, Esq. (Being an abstract of a Paper read at the Western Medical and Surgical Society, January, 1860.)

The necessity of attention to the respiration was considered chiefly of value—1st, in coma, from injury or disease, sanguineous or serous; 2ndly, in narcotism of the brain; 3rdly, in epilepsy; 4thly, in certain functional cerebral derangements, where a tendency to headache and lethargy exists.

The strictly anatomical connexion of the lungs with the brain having been alluded to, it was shown how, pathologically, disease of either of these organs might kill by its effects upon the other.

1st. Cases of coma from cerebral injury and apoplexy were then cited, illustrating how death generally took place by apnæa. The practical point, therefore, indicated was that life might not only be prolonged but saved by attention to the respiration, even in cases apparently the most hopeless.

2nd. In narcotism. The greater the insensibility in narcotic coma, the more the respiration is affected; the more therefore it requires to be watched. Coma and narcotism both kill by the lungs; but several points of difference were shown to exist between the apnœa of coma and that of narcotism. In the apnœa of coma, death is not generally (or at all events primarily) due to cessation of respiratory action—*i.e.*, muscular paralysis, but to lung paralysis, evinced by extreme and rapid congestion of the lungs, accompanied by rapid effusion into the pulmonary air-cells and bronchial tubes. The death is apnœa by effusion, unless it can be warded off. The treatment for the apnœa of coma is venesection, to relieve the congestion of the lungs, and prevent its further formation; but more especially important is the prone position (the prenopnœa of Dr. Marshall Hall), by which an enormous amount of bronchial effusion may be got rid of. Without the prone position, the bronchial effusion may so accumulate that the patient with coma may, in reality, die of narcotism from non-eliminated carbonic acid gas. Artificial respiration may not be necessary. A case of apoplexy of the medulla oblongata and one of compression from injury were related, in which no muscular paralysis existed. In the apnœa of narcotism there is neither the rapid effusion nor the great congestion of coma; but if death ensues, it is chiefly from musculo-respiratory paralysis. The pulmonary congestion is less than in cases of coma, for the narcotic influence diminishes the strength and frequency of the cardiac pulsations, which are unrestrained in coma. In the treatment of narcotic apnœa, venesection is not necessary, nor is prenopnœa, to remove effusion, but it is especially necessary for a different reason—viz., lingual paralysis. Artificial respiration is here invaluable. If employed, however, without attention to the tongue, it may be useless.

3rd. In epilepsy. The normal state of the respiration is a point in the etiology of this disease requiring, in the author's opinion, careful consideration. Many epileptics have a shallow, contracted, and feebly-acting chest, which predisposes to and must keep up the epileptic tendency. Several patients were alluded to whose respirations were not only very limited in amount, but in number also, being less than one to four cardiac pulsations. This feeble lung action acts injuriously to the epileptic in a two-fold way; it tends to retain carbonic acid in the blood (to which gas many of the symptoms peculiar to the epileptic, besides the seizure, seem attributable); and also prevents the lungs acting freely as diverticula to the cerebral circulation.

4th. Amongst the functional cerebral derangements were mentioned headache and lethargy, which free respiration in the open air would often remove; also cases in which respiration would sometimes, without any warning, become extremely difficult (as if from sudden deprivation of nervous influence); in such cases, fresh air, the inhalation of ether and ammonia, and deep inspirations (forced, if the patient can effect them, assisted, if not), appeared to the author to be indicated, and were productive of great benefit in these conditions.—*Laecet, Feb.* 4, 1860, *p.* 120.

20.—ON THE RATIONAL TREATMENT OF DELIRIUM TREMENS.

By Professor Dunglison, Philadelphia.

[In a paper published some time ago, (Retrospect vol. xxxviii, p. 36), Dr. Laycock asserted his opinion,]

That the delirium and sleeplessness indicate comparatively harmless conditions of the nervous system; that they are usually symptoms of some disease occurring in persons of drunken habits; that they usually cease within a given time, spontaneously; and that the proper method of cure is to treat the general or particular morbid state, whatever that may be, with which the delirium is associated. He further showed that opium and alcoholic stimuli, as generally recommended for this disease, were not only useless but dangerous drugs.

[The present article is a portion of a letter by Professor Dunglison, to Dr. Laycock: it shows that the two observers had come to the same conclusion independently of each other. In May 1842, Dr. Dunglison as Editor of the American Medical Intelligencer, inserted an article in that Journal on the "Eclectic treatment of Delirium Tremens."]

We stated that the course pursued by us in the treatment of delirium tremens has been entirely eclectic, in many cases expectant, and that the results have been such as to satisfy us. Under the view which we entertain of the nature of the affection—that the irregularity of nervous action is usually induced by the withdrawal of an accustomed stimulus, and that the recuperative powers are generally entirely sufficient to bring about the necessary equalization—we have treated the mass of the cases which have fallen under our care without either excitants proper, or opiates. In the first instance, an emetic is given at times, if the patient is seen whilst labouring under the effects of a debauch, or any particular reason exists for its administration; and afterwards, a state of tranquillity in the chamber is enjoined—the intrusion of too much light and noise being prevented; and, when the stomach will retain it, gently nutritious and easily digestible diet is prescribed, the bowels being kept open by gentle cathartics;—and this has comprised the essential part of our treatment. In time the hallucinations have disappeared, sleep has returned, and entire restoration supervened. The preceding remarks are a proper prelude to the statistical account of the Women's Lunatic Asylum, at the Philadelphia Hospital, for the years 1840 and 1841, which is under our charge during the six months commencing on the 1st of November, and ending on the 1st of May, and under that of Dr. Pennock for the other half of the year. It may be proper to add, that, since 1st of November 1841 to the present time (May 1, 1842), not a drop of alcoholic liquor has been used in the treatment of delirium tremens in the Women's Asylum, although some severe cases in the third stage have occurred, which, notwithstanding, terminated most satisfactorily.

Patients admitted into the Women's Lunatic Asylum of the Philadelphia Hospital, in the Year 1840.

		Cases admitted.	Cured.	Died.
Intoxication,	. .	25	25	...
Delirium Tremens,	1st stage,	34	34	...
Do.	2d stage,	10	10	...
Do.	3d stage,	4	3	1

The fatal case was not seen by us. The patient died on the morning after her admission into the hospital, and had been treated in the city for nearly a week previously.

Year 1841.

		Cases admitted.	Cured.	Died.
Intoxication	. .	19	19	...
Delirium Tremens,	1st stage,	21	21	...
Do.	2d stage,	9	9	...
Do.	3d stage,	6	6	...

In the third edition of my "*Practice of Medicine*" (Philadelphia, 1848), I state further: "A more recent authentic abstract of the number of patients admitted into the same asylum, from the 1st of November 1844 to the 4th of February 1845, exhibits that 32 cases were received, 18 of which are classed as intoxication. Of these not one died. The treatment here again was eclectic, often expectant, and not a drop of alcohol was given."

The results of the above plan of treatment were referred to some time ago, in an interesting pamphlet on *Rational Medicine*, by Professor Worthington Hooke, of New Haven; but as I had doubts whether either the *Medical Intelligencer* or my *Practice of Medicine* is in the libraries of Edinburgh, I have copied from them what bears on the rational view which you have embraced of treating delirium tremens. As I have remarked in the latter work: "It has, in the first place, restored the individual to health, not, perhaps, as rapidly as either brandy or opium, but more permanently. The term, 'restoration to health,' is hardly, indeed, applicable to the change effected by the former remedy. The patient is merely placed in the condition in which he was before the stimulus was withdrawn; and as he was 'restored' by the brandy, he is apt, as before remarked, to regard it as indispensable to his healthy condition. In the 'total abstinence' plan, however, the habit of drinking is broken in upon; and even if it should require a short time longer to restore the individual, there is the consolatory reflection, that delay is not useless, and every day's privation of the wonted stimulus diminishes the feeling of necessity, and the desire for it. One evidence of the good effect of the course is, that they who are dismissed cured rarely or never return to the wards. This is an observation that has been made at the Philadelphia Hospital; and as it concerns paupers, it is probable that the cures are real and permanent, for, were it otherwise, they would, in subsequent attacks, be compelled, in their destitution, to seek the wards of the same excellent charity."—*Edinburgh Med. Journal, April* 1860, *p.* 924.

21.—ON THE PATHOLOGY AND TREATMENT OF IDIOPATHIC PERIPHERAL NEURALGIA.

By Harry Lobb, Esq. (Read before the Harveian Society.)

The author commenced by dividing neuralgia into (1) Central, (2) Peripheral, and (3) Reflected. Of the central, arising from disease of the brain or spinal cord, he did not speak. The third division he proposed reserving for a future communication. The second was divided into Idiopathic, Traumatic, and Neuromatous. Idiopathic peripheral neuralgia was described as a stabbing, darting pain referred to the course of a nerve, shooting down the nerve like lightning, coming on suddenly, lasting but a moment, and repeated at intervals; the pain is so acute as to be unendurable if continuous. The part or limb affected with this form of neuralgia is colder, and the skin supplied by the neuralgic nerve is more or less numb, not tender to the touch, the patient liking to be rubbed, and frequently grasping the part with the hands and pressing it. If the neuralgia has lasted any time, there is more or less paralysis in the muscles supplied by the accompanying motor nerve. The author then gave, at some length, his views upon the generation and distribution of nerve-force from the capillary circulation, in order to make his description of the pathology of this form of neuralgia intelligible. He described a sentient nerve as a conductor to the brain of sensations taking place at the periphery: neuralgia is not, therefore, a hyperæsthesia of its healthy function. During health, a nerve has no sensation proper in itself; and if it be struck, cut, or torn, the sensation is referred to the parts to which it is distributed: but in this form of neuralgia it is the nerve itself to which the pain is referred as darting up and down its course. A sentient nerve was likened to an iron conducting wire of a galvanic battery, which, if of a certain uniform diameter, conducts a given quantity of galvanism without being perceptibly affected by its passage; but if a portion of the wire be much finer than the rest it becomes red hot, being unable to conduct the whole of the galvanism, the remainder correlating into heat. So in a nerve: from mal-nutrition, it is unable to conduct normal sensations to the brain; the nerve-current, by affecting the polarity of the nerve itself, gives the idea of pain in that portion of the trunk of the nerve, its peripheric terminations at the same time being numbed. The author considered the indications for treatment, therefore, to be increased and healthy circulation, arterial and nervous. After enumerating the usual methods of treatment, he considered the only agent capable of carrying out these indications to be the continuous galvanic current; and he recommended, as the only apparatus that can be adapted to the surface of the body, and capable of generating a sustained current, the Pulvermacher chain—producing a continuous current of galvanic electricity in one uniform direction, mild, yet sufficiently energetic for medical purposes. Mr. Lobb then described the method of applying the chain, and exhibited some experiments with its aid; as the decomposition of water, contraction of muscles, &c. In the treatment of idiopathic peripheral neuralgia, he looked upon the Pulvermacher chain as a specific. Immediately upon adapting the excited chain to the part, a genial glow is felt—not mere warmth, but a sense of vitality in the part; the patient is aware of a life-giving agent, and immediately says that he is relieved; the neuralgia disappears, and sensitiveness of the surface returns.—*Lancet, Dec.* 10, 1859, *p.* 589.

22.—ON THE PHYSICAL CONDITION OF THE MUSCLES DURING MYALGIA.

By Thomas Inman, M.D., Physician to the Liverpool Royal Infirmary.

[Dr. Inman has, in several previous papers, (see 'Retrospect,' vol. xxxviii, p. 19, and vol. xxxvix, p. 87) drawn the attention of the profession to the fact, that a vast number of symptoms once supposed to indicate inflammation of internal organs, rheumatism, neuralgia, hysteria, and a variety of other diseases, are due in reality to painful affections of the fleshy or tendinous parts of muscles. In myalgia from excessive exertion, various appearances are observed in the muscles. In the bodies of those who have died from tetanus,]

Microscopic inquiry shows that their blood-vessels have been emptied, that many of the muscular fasciculi have been ruptured, and that each of these fractures is attended with laceration of blood-vessels, and extensive extravasation of blood between and around the broken fibres.

[Dr. Inman then gives an account of an examination of a hare which had been coursed to death; the fibres were lacerated; blood effused between them, and between the ruptured ends of the fibres, and the transverse markings almost gone.]

This, then, being the condition to which the muscles of a hare are brought from excessive action, it is not straining analogy too far to assume that in a man who had overstrained his muscles as "poor Puss" had done, a similar state of things would be found, had we an opportunity for examining him; and it is easy to understand how such a condition would be attended with cutaneous tenderness, prolonged soreness, great disinclination to use the bruised organs, and severe suffering if the action was continued daily. We can readily see, with this physical condition of the muscles, how inflammation might readily supervene, in cachectic constitutions, upon the injury done, as an abscess occasionally follows a bruise, and how it would be difficult for the muscles to recover their healthy state without a large amount of rest, or a great reparative power in the system, or a long apprenticeship of pain. If the foregoing reasoning be valid, it is equally valid to assume that an amount of exertion, less than that we have described, might produce an analogous state of things, though of less severity. If so, we may conclude that in such myalgic cases as we have previously described. there is less or more rupture of muscular fibre, laceration of blood-vessels, and extravasation of blood.

It must next be noticed that the above phenomena are due to *excessive* muscular action, and we must pause to inquire what is the signification of those words. In a previous communication, I have endeavoured to prove that the standard by which *excess* must be judged is not the general average of human exertion, but the powers of the individual—*e. g.*, a daily walk of twenty miles is not excessive exertion for a country postman, while he is healthy, but a walk of twenty paces is excessive exertion for that postman when ill with typhus. A feeble woman, therefore, is quite as obnoxious to suffering from over-exertion, though she may never leave the house or her bedroom, as is a healthy man or a hunted hare.

Our chain of argument would be strengthened if we could show that the muscular fibres will fracture and the blood-vessels give way in the weakly, under a less amount of force than they do in the strong. I am unable to give demonstrative proof of this, and must content myself with the best available analogy.

1. Toleration of muscular exertion is in direct proportion to a muscle's *tone*—*i. e*,, firmness, hardness, and development. In strong muscles, "soreness" is unknown, except from such very prolonged exertion as "pumping at sea", a thirty hours' "march", a long day at the oar, &c.

2. Myalgic pains are severe in the direct proportion to the atony or softness of muscles; they are, therefore, most common in those whose system has been reduced by cachexias of various kinds.

3. Experience in the dead house and dissecting-room tells us that the muscles of certain corpses give way sooner than those of others; *e. g.*, it is not an uncommon thing, when dissecting the upper extremity, to find that both the pectorals are torn through by the simple

weight of the arm. This easy rupture occurs only in those dying of cachexia. In such corpses, too, it is that the blood-vessels are commonly broken by "injection". We further know that those hearts are most liable to rupture whose tissue is pale, soft, and friable, and whose fibres are indistinctly marked with striæ; and we also know that when cardiac rupture does occur, it is the result of very trifling over-exertion, if there can be said to be over-exertion at all. The ecchymosis under the skin, and the hardness and other phenomena characteristic of myositis in the legs of scorbutic seamen, lead us to infer that a condition of things exists in them resembling that recognised in the tetanic patient or the hunted hare; and if it do exist, it cannot be from absolutely excessive muscular action, but from exertion disproportionate to the contractile power.

4. Experience amongst the living tells us that the blood-vessels are sooner ruptured in some persons than in others; that is to say, that some will have ecchymosis from a touch, a pinch, a blow, or other trifling injury; while others will endure far more violence without any extravasation of blood whatever; and, if we classify these, we shall find that the tendency to "breaking of blood-vessels" is great or small according to the cachectic condition of the individual, and *vice versâ*. Thus, accidental hemorrhages into the muscular, mucous, submucous, cutaneous, and subcutaneous parts, are common in scurvy, in purpura, and in typhus; they are not uncommon in phthisis, scarlatina, Bright's disease, and general asthenia; they are very rare in the healthy countryman or robust sailor. It is amongst the former that we find myalgic pains are the most severe, prolonged, and intractable. If, then, we find that the cachectic are, *cæteris paribus*, far more liable to rupture of muscular fibre and of blood-vessel than are the strong, we are justified in the belief that such fracture will occur from a smaller *absolute* amount of muscular effort in the former than in the latter (the *relative* muscular effort is perhaps *greatest* in the weak); consequently, that there is nothing improbable in the belief that *excessive* exertion in the delicate and feeble, even though the effort seems to us very trifling, will produce similar painful effects to excessive exertion in the strong. But it is a physiological fact of great interest, that, as a general rule, the weakly suffer pain from any given cause to a far greater extent or proportion than the strong; *e. g.*, a bad tooth does not trouble us at all while we are well; but if we get "pulled down" in any way, that same tooth will produce both odontalgia and neuralgia, both of which complaints may be removed by full doses of quinine or other tonic, which builds us up again, the bad tooth still remaining the same. Consequently, we judge that the muscular and vascular ruptures, which are the cause of suffering in the strong, are the cause of a proportionably greater suffering in the weak.

If the preceding account of the physical changes produced in muscles from excessive exertion be accepted, it disproves most completely the idea of Dr. Handfield Jones that the pains and other phenomena of myalgia are simply "neurolytic" and dependent on "nerve-debility";

and that of others, which attributes them to hysteria, feminine fancy, or the desire of sympathy. It gives us a good ground for the belief that the sufferings described are *real*; and it unequivocally points to a plan of treatment of which practice has already proved the value; viz., that the muscles should have time to rest, and thus be enabled to repair fractured fibres, and "take up" extravasated blood; it leads us to expect that in cachectic subjects, ecchymosis may pass on to inflammation, and this to abscess or disorganisation; and it still farther points to the necessity there is, in all cases of myalgia, for improving muscular "tone" and increasing vascular tenacity. It demonstrates that, as a general rule, champooing, friction, galvanirm, or other means which would aggravate muscular exertion, blisters or other counter-irritants, or leeches or local blood-letting, cannot be expected to do more good than they do in common ecchymosis.

In conclusion, I would only add, that the preceding remarks apply to myalgia of the fleshy portion of the muscles alone, and are not applicable to the pain, &c., which accompanies a prolonged strain upon their fibrous parts.—*British Med. Journal, Feb.* 4, 1860, *p.* 87.

23.—CASES TREATED BY THE HYPODERMIC INJECTION OF ATROPINE.

(Under the care of Dr. Cowdell, Dorset County Hospital.)

The following cases I beg to submit to the notice of the Profession as evidences of the great practical value of the hypodermic injection of atropine in sciatica, lumbago, and, in fact, in all external aches and pains. No claim to originality is made, since it is Dr. Wood's method, as modified by our French brethren, and reported in the 'Journal de Medécine et Chirurgie Pratique.' Dr. Wood's morphine injection has been practised on several patients without the results now obtained from the use of atropine. The morphine procured sleep in from three to ten minutes, but when the narcotic effect had passed off, the pain returned as bad as before. The atropine causes slight drowsiness, dryness of the throat, and dilatation of the pupil: the only symptom of moment being the disappearance of the pain. The sulphate of atropine being more soluble than the alkaloid itself, is preferred; the strength of the solution is gr. ij. to ℥j. of water. The instrument employed is an ordinary nævus injecting syringe, consisting of a graduated tube with screw piston, and a hollow needle which screws on to the tube. The needle is thrust under the skin as near the seat of pain as possible, and from ten to thirty minims of the solution gradually injected.

The cases subjoined will show the result:—

Case 1.—S. E., aged 20, a domestic servant, admitted September 29th, 1859, and stated as follows:—"Had an attack of acute rheumatism five months since, was delirious at intervals during three weeks, and, when recovering from the rheumatism, sixteen weeks ago, had an acute pain in the hip, which did not yield to the leeches or blisters which were repeatedly applied." On admission she was extremely anæmic, and experienced acute pain in the region of the sciatic nerve, increased by pressure. There was no hip affection: contour of hip normal; muscles very flabby; hamstrings contracted; knee bent; and the foot drawn up. She had during the whole time been unable to walk without crutches. Ordered meat diet. Ol. morrhuæ and a draught containing iodide of potasium.

30th. Passed last night as usual, without sleep, and in agonies of pain. Injice sol. atropii. sulph. ℳxxx. in par dolent.

October 1st. Slept well all night; felt immediate relief after the injection; says she has not had so good a night for sixteen weeks.

4th. Hip is sore, but no pain unless pressed. Repet. inject. horâ somni.

5th. The pain is entirely gone, not even the soreness remaining. Omit. mist. ℞ Ferri. am. cit. gr. iv. ex aq. ter die.

She was treated for anæmia from this date to November 10th, when the report runs thus:—

"November 10th.—Patient has had no pain in the hip since the last injection on October 4th. Two injections only were employed, at an interval of three days, the first of which gave immediate relief, the second removed pain and soreness altogether. Can walk with ease; no pain on pressure over the nerve, and she gains flesh rapidly. Discharged well."

Case 2.—H. P., aged 25, plasterer, admitted with chronic rheumatism, December 1st, 1859. He was somewhat relieved by the use of guaiacum and iodide of potassium, which was continued until January 12th, 1860, when complaining of an additional pain in the lumbar region which caused him to walk with his hands on his knees, the atropine injection was used.

January 13th. The pain in the back was greatly relieved last night by the injection. Omit. omn. meda. ℞ Syr. ferri iodid. ʒij. ter die.

15th Very much better; no pain in back; can walk quite erect; has a pain in the tensor vaginæ femoris muscle. Inject. repetatur.

17th. Pain gone from the tensor vaginæ femoris muscle, but experiences slight pain in the gracilis and inner hamstrings. Repet. inject.

26th. Feels well and able to work; no pain whatever; says he is cured by the injections. Discharged well.

Case 3.—E.D., aged 50, nurse, admitted January 9th with all the ordinary symptoms of acute sciatica of one day's standing. She could not stand. Agonizing pain in back and hip, and leg as far as the great toe. Ordered cal. cum opii. and the atropine injection at night.

13th. Found much benefit from the injection last night, leg much better, but still a pain on pressure.

14th. Used injection again last night, and this morning the pain is entirely gone; no pain even on pressure.

19th. She continued improving, and on this day she was discharged well.

The peculiarity in this case is, that she was admitted the morning following the commencement of the attack; and therefore it is worthy of notice that neither abstraction of blood, nor counter-irritation was resorted to, but the injection of atropine only, the effect of which upon the disease is remarkable.

Case 4.—W. S., aged 38, labourer, admitted February 9th, 1860, with sciatica on the right side, which had existed since October, 1859. He walked, or rather limped, into the ward with both hands resting on a stick between his legs, dragging his right leg after him. Pain was increased by pressure over the nerve, from the lower leader of Glutæus maximus muscle to the popliteal space. Ordered the atropine injection at bed-time, and a mixture of iodide of potassium.

10th. The injection last night removed the pain; only an ache is experienced to-day, instead of the shooting pain of yesterday. Repet. inject. h. s.

11th. Feels no pain or soreness whatever in the hip. Pressure even into the sciatic notch gives no pain, and he can walk with as much freedom and ease as before the attack. He was discharged well on the 16th, seven days after admission.

Case 5—D. S., aged 56, stone-mason, admitted February 9th, 1860, wit sub-acute sciatica of fourteen day's duration. Walked with difficulty even with the assistance of a stick: could not bear the pressure of a finger over the nerve. He had also bronchitis of the larger tubes, accompanied with profuse expectoration. Ordered the atropine injecton at night, and a squill and henbane mixture for cough.

10th. Cough less troublesome; the injection gave immediate relief; very little pain is now experienced even on pressure over the sciatic nerve, and he can walk without pain, but has a limp. Continue.

12th. Has now no pain in hip even on pressure; walks well and with ease; cough better.

From this day to the 23rd he was treated for his chest affection, being still free from pain in the hip. Discharged well on the 23rd.—*Med. Times and Gazette, March* 17, 1860, *p*. 267.

24.—A CASE OF POISONING BY STRYCHNINE, AND RECOVERY.

By George Bennett, M.D., F.R.C.S., Sydney, New South Wales.

[The patient, a lady aged forty-two, had, in a fit of despondency, taken the entire contents of a packet of "Battle's rat poison." Then repenting of what she had done medical advice was sought. Dr Bennett administered scruple doses of sulphate of zinc, whilst he sent for the stomach pump, but by its arrival the spasmodic movements which had supervened precluded its use. The compound tincture of iodine was then sent for.]

Before it arrived, however, the spasms had increased so violently that life appeared extinct; the face and hands were livid, the eyeballs protruded, and there was violent opisthotonos; pulse not perceptible. The most severe spasmodic action came on about a quarter to eight A.M., and about two minutes after the subsidence of a severe fit, the medicine having arrived, I gave her twenty drops in a wine glass of water. Soon after a very violent fit came on, and in this the respiration was so feeble that we all considered she was dead. However, she again rallied; and as soon as she was capable of swallowing, being about ten to fifteen minutes after the administration of the first dose, I gave her thirty drops of the tincture of iodine. She soon had another fit; but it was evident to all about her that it was less severe, and lasted but for a very short period of time. On the fit subsiding, in about ten minutes I gave her another dose of thirty drops of the tincture. The convulsive fits had now ceased, and in about twenty minutes from the cessation of the last fit, violent vomiting came on; and this being assisted by diluents, the stomach appeared clear of the poison. Now, a question may arise how the iodine acted in this case. Probably by forming an insoluble compound of hydriodate of strychnine, and by relieving the system from the spasmodic action of the poison upon the spinal nerves, the emetics which had been administered were enabled to take effect. She continued free from any further spasm; and, after an hour, I left her. On returning in about an hour, I found she still continued better, with the exception of a few very slight twitchings. She complained of great debility; and the pulse being very feeble, I ordered her nourishing diet of milk and arrowroot, and gave an ammonia mixture, with compound sulphuric ether; and, under their influence, she rallied very rapidly. I remarked that during the convulsive paroxysms no screams were uttered; and on making inquiry after her recovery, she informed me that no pain was felt during the fits, only an inconvenience from the spasms drawing the legs so rigidly down. She also stated she was sensible during the fits to every thing around her, retaining the most perfect consciousness of all that was said or done—was unable to speak, but never felt any inclination to scream or cry out. In the evening she complained of a little soreness of the throat, and a slight feeling of cramps over the lower extremities.

On the following morning (Oct. 14), I found her much better; pulse regular; had passed rather a restless night; soreness of the throat had subsided; and she enjoyed some tea for breakfast. She had no pain, and complained only of a feeling of lassitude. The next day I found she had slept well; but the bowels not having been relieved, an aperient mixture was ordered, which, however, did not operate until the evening. The stimulating mixture was continued occasionally, and afforded great relief from the feelings of nervous debility which came over her. From the 16th to the 18th she improved daily; and on the latter day she was able to leave her room.—*Lancet, Oct.* 29, 1859, *p.* 434.

DISEASES OF THE ORGANS OF CIRCULATION.

25.—ON FIBRINOUS DEPOSITION IN THE HEART.

By BENJAMIN W. RICHARDSON, M.A., M.D., Lecturer on Physiology, at the Grosvenor Place School of Medicine, &c.

[In a previous article, Dr. Richardson considers the structure of fibrinous deposits, and the proofs that these deposits are in many instances laid down prior to death. In the present paper he passes on to consider those conditions of the system which are favourable to the deposit of fibrine, or, in other words, those diseases in the course of which these deposits are formed. To understand this subject clearly, it is first necessary to consider the modifications to which the fibrine of the blood is subjected under various diseased states.]

1. In various conditions the fibrine is, or seems to be, diminished in the blood. These conditions are indicated in diseases in which the hemorrhagic tendency prevails. True typhus, and yellow fever, are diseases of this nature. The effects of many poisons, as the cobra poison, the vegeto-alkaloids, the alkalies, and several organic acids, are manifested in this apparent deficiency of fibrine. In these cases, the blood, as Plato would express it, "is no longer under the natural guardianship of its fibres; so it exudes into the soft structures, giving rise to petechial spot, black vomit, melæna, hematuria, and, in fact, to any form of hemorrhagic discharge, according to the degree of pathological change.

I have said that, in these cases, the blood seems to be deficient in fibrine. I say *seems* to be deficient, because I wish to guard myself from saying absolutely that the blood is so deficient; and I would impress this qualification earnestly, because, from some late inquiries on the effects of alkalies and alkaloids on blood containing, when drawn, the normal amount of fibrine, the same apparent decrease of fibrine has been produced by the mere addition of these agents in excess to the blood. We need not, therefore, bind ourselves to the idea that, in diseases attended with great fluidity of blood, less fibrine is really produced in the system; we may rather infer that, as all evidence tends to prove, there are conditions in which the blood is surcharged with a body having the power of holding fibrine in solution—of reducing the fibrine, in fact, into a form of albumen. In such condition of blood, whether my explanation of its cause be correct or not, this that I am about to say shall be correct; the blood, while in the condition named, never, either before or after death, yields a fibrinous separation. Ordinarily, it does not coagulate even as red clot, but at most it becomes a thick treacly mass, like newly drawn blood treated with an alkali and afterwards mixed with albumen.

2. There is a second variety of blood, not widely removed from the above-named kind, in which there is nevertheless a peculiar tendency to separation of fibrine; and this without any necessary increase of

the fibrine itself, or the presence of those systemic states in which the combustion of the body is increased. The older writers were accustomed to notice this separation of fibrine in scurvy. They spoke of the stream of blood in that disease as having two colours, a purple and a white; and of a loose buffy coat forming on such blood. I am convinced to proof that the condition here expressed exists—exists sometimes, as it would seem, by hereditary predisposition; at other times, by acquired disease, especially by disease derived from peculiarities of food. I do not pretend to be master of all the facts connected with this subject, but I will narrate such few as I know. I once attended a child covered from head to foot with purpuric spots—a miserable puny child, cold always, and emaciated. This child died suddenly, and I traced the cause of the sudden death clearly enough to a large concretion of fibrine which had been laid down in the right auricle. I also knew an instance of extreme anæmia terminate in a similar way; the only difference being, that the concretion had formed in the pulmonary artery, and not in the auricle. In a case of purpura at this very time under my observation, there is every disposition to effusion of blood under the skin, and large purpuric blotches are produced from the slightest cause; but, sometime after this effusion has occurred, there is a hardening of the matter at the effused point; and it is observable that the blood of this patient, although it is so easily effused, is not incapable of coagulation when it escapes from the body.

3. There is another condition, in which the fibrine of the blood undergoes relative increase. It may be difficult at first to understand what is meant by relative increase. I mean by it then, simply a state in which the fibrine is not abnormally increased, nor the system under any condition for the production of an increase of fibrine, but in which, the fibrine remaining normal, the watery constituent of the blood is below the usual standard. This condition is not uncommon; for, water being the grand menstruum in which the fibrine is held in solution, any reduction of it in amount will favour materially the tendency to deposit. It may seem, at first sight, that this statement runs counter to that which was made above relative to anæmia; but there is no contradiction. Let me explain. Water is, us I have said, the menstruum in which the fibrine is held in solution. But fibrine will not remain in solution in water simply; the water must be the menstruum also for an agent having the power of dissolving the fibrinous constituent. This agent, as we now have proof beyond every kind of dispute, except sheer cavil, is an alkali of the volatile series. Now, in order that fibrine should remain in solution, there must be a fixed relationship existing between the menstruum, the solvent, and the substance dissolved. If the substance dissolved is but dissolved, and no more, as fibrine is in blood, precipitation of fibrine can be induced by addition of more water, because, in this addition, we distribute the solvent over a larger surface; we weaken, that is to say,

the solvent power of the solution; and the result necessarily is, that the dissolved substance is proportionately precipitated. On the other hand, if from a solution of fibrine, as in blood, we draw off by osmotic action the watery part, together with the solvent, the fibrine is necessarily increased in relation to the portion of water and solvent left behind. In both cases alike, there is, therefore, a tendency to the deposition of fibrine. Out of the body, I can produce in experiment all these phases; and in the bodies of the dead we constantly meet with the results of these changes, if we are observant observers.

The best marked cases in which the fibrine is found separated after death, as the result of loss of fluid from the system, are those where, during life, there has been an exhausting and rapid flux from the alimentary canal. In cases of cholera, the fibrinous deposit so commonly found after death is formed in this way; and the same may occur in other disorders affecting the alimentary system. I have seen them, as so produced, in death from mesenteric disease; and once in a debilitated person, after a fatal purging, induced primarily by self-dosing and overdosing with black draught. Deposition, brought about by loss of fluid, will further happen from rapid elimination through other channels than the bowels, as from the skin in the colliquative sweating of phthisis.

Once more; the fibrine may be relatively increased, not in proportion to the water, and not in proportion to the mass of blood, but in proportion to its own solvent. The old writers were accustomed to state that concretions were the result of what was called languor of the circulation. We cannot adhere to that phraseology at this time of science, because the phraseology is imperfect; but we may, nevertheless, accept that these writers did truly express a partial explanation of a very interesting fact. In many cases of great prostration of the body and of the powers of the circulation, where, notwithstanding, life is slowly destroyed, the blood making languid way for many hours —in such cases (and I may add, in the majority of such cases), the fibrine is left deposited in the heart. The cases themselves include: *(a)* deaths from shock, where the life is not extinguished at once, but in which the act of death has been very considerably prolonged, say for a period of from three to four days; here the fibrine may be separated in a firm and decided mould; *(b)* deaths from the prolonged effect of some poisons, which depress the heart without adding any solvent substance to the blood, such as opium; and *(c)* those instances of disease where death is preceded by great degenerative change, and by slow and certain exhaustion. I have met with *ante mortem* concretions in examples of this nature, after death from mere senile decay, fatty degeneration of the heart, and slow alcoholic poisoning. The concretion, however, in these cases, is often but indifferently developed. It is gelatinous in character, very heavy at first from the presence of water, but containing in reality a very small amount of fibrine. Dr. Gould, whom I have noticed once before, laid great stress on debility as a cause of deposition. He opined—and the opinion has been reiterated by Dr. Meigs of Philadelphia—that, owing to the stasis produced in the blood during a fit of syncope, the commencement of a fibrinous deposition may be traceable, in some instances, to an attack of syncope. Nor is it to be denied that many arguments lend a colour to this view of stasis as a cause of concretion. The observation made by Lancisi respecting the mode in which fibrine is deposited in aneurism, the first point of deposit in the sac of an aneurism being that at which the stasis is most marked, was alone of sufficient importance to establish the stasis hypothesis for a long period; and the more so, because this hypothesis is not opposed to the truth, but is only faulty in that it does not express the whole truth. The position of the question is as follows:—When blood yet fluid is placed entirely at rest in a closed cavity, and under a pressure equal to that of the circulation, there is no separation of fibrine. On the contrary, blood so enclosed is in the best condition for its fibrine to remain in solution. Here is an argument, then, in direct opposition to the pure stasis view; flatly disproving it, indeed, as a bare hypothesis. Blood, therefore, from which *ante mortem* concretion is formed in the asthenic, is not blood in a state of absolute rest. No; it is blood undergoing slow and languid motion, in process of which the volatile solvent of the fibrine, a product of an active nutrition, is in the first place imperfectly supplied, and, secondly, is eliminated in the expirations of the skin and lungs, to the loss of the blood. The relationship existing between the fibrine and its solvent being thus destroyed, the fibrine is precipitated, and is found in the separated condition after almost all forms of death from asthenia, excepting those where a solvent poison is present as part of the disorder.

4. The last condition favourable—most favourable, to fibrinous separation, is that in which the blood undergoes an absolute increase of fibrine—that condition which the old men were content to designate as disease "attended with sizy blood," and which we moderns, following Franz Simon, call "hyperinosis" or superfibrination. We include under this head a large group of diseases marked by inflammatory symptoms, and always running an acute course; pneumonia; croup; diphtheria; acute rheumatism; erysipelas; inflammatory diseases of all the serous and mucous tracts; the inflammatory reactive fever succeeding on surgical operations; inflammatory lesion in the uterus and peritoneum after parturition; and cerebral congestive excitement.

In thus naming certain local symptoms indicating what we are pleased too often to consider as pure diseases, I write to make the subject intelligible. In my opinion, these local changes are all secondary to one grand systemic derangement, a primary and essential feature of which is increase of fibrine—hyperinosis.

Still further, I maintain that there are cases in which the hyperinosis is not only preexistent to any local manifestation of disease, but in which it is so marked, and the deposition of fibrine in the heart as a consequence of the increase is so rapid, that death takes place from the obstruction, before any local change is developed. Instances of this nature, while they are not by any means numerous, are, as a general rule, overlooked. There are, nevertheless, to be found in our literature certain extraordinary examples of this nature, worthy certainly of notice. One of the very best observers of disease in the last century, Dr. Huxham, in a letter written by him to Dr. Mortimer, describes what was sometimes called an epidemic of cardiac polypi. In this letter, it is narrated that several seamen, brought during cold weather into England from the West Indies, were seized with short coughs without expectoration, continual and violent palpitations of the heart, intermitting trembling pulses, anxiety, and pain and sinking at the heart, so that they could not lie down; they had heavy dead countenances, some pains in the side, but very little fever. Twenty men thus soon died. Two of these men were opened, and in their hearts were found monstrous polypi, tough and adherent to the walls of the ventricle. One of these weighed an ounce. They were on both sides the heart; the largest on the right side.

A still more remarkable illustration of a similar epidemic is described in the *Edinburgh Annals of Medicine* for the year 1800, by another equally good observer, Dr. Chisholm. I have often noticed this valuable paper, but now I will give it almost entire. It is entitled "A Short Account of the Epidemic Polypus in Granada in 1790." The sufferers were negroes, who were subject to peculiar local influences at the time of the seizure. These influences Dr. Chisholm first describes. The foreground was the open sea, with an extensive and bracing beach of sand. On the left was a hill of considerable size and ascent, the base sides of which, with the reflecting surface of the sea, produced in dry seasons an immense degree of heat. The background was a marsh, extending from the sea to the mountains. From this marsh deleterious vapours were exhaled. Immediately behind the marsh, a ravine began, through which rushed a current of wind of great coolness. The negro houses of the plantation were built on the hill on the left, chiefly on its slope and towards the edge of the marsh. The negroes were, therefore, at once exposed to excessive heat, a cold chilling current of air, and the miasma of the marsh. Their diet was chiefly vegetable food. They had been employed, immediately before the appearance of the disease, in clearing the surface of the marsh, and in holing land for the reception of cane-plants. They were much given to the destructive habit of eating a species of pipeclay very abundant in Granada.

The decease commenced in the end of September, was most prevalent in the latter part of October, and went off in November. The whole number of the sick was about forty, of whom seven died.

The symptoms were, pain at the pit of the stomach and head, and difficult respiration, attended with dry skin, small quick pulse, and slight dry frequent cough. No febrile heat accompanied these symptoms. The surface, on the contrary, was remarkably cool; but there was a heaviness and dulness of the eye, anxious features, and depressed spirits. This state continued for three days; then the pulse became quick, from 120 to 140, and intermitted, attended with a penetrating pungent heat, which produced a pricking sensation on the hand of the person feeling the pulse.

The disease now became somewhat intermittent, the intermission lasting eight or nine hours. During the paroxysm, the struggle for breath, the aggravation of all the other symptoms, and *the very quick*, interrupted, and evidently visible, as well as audible palpitation of the heart, produced a scene of uncommon horror. The paroxysm was succeeded by a cold clammy sweat, and a state of approaching syncope. The second paroxysm generally put a period to the existence of the patient. The disease was also distinguished, during the latter stage, by an almost constant, disagreeable clammy sweat, overspreading the face, the upper extremities, and the body as low down as the scrobiculus cordis, all below remaining parched in a most remarkable degree. The disease seemed sometimes inclined to terminate by a metastasis. One instance of this was remarkable, wherein a patient, after labouring under all the symptoms peculiar to the disease before the intermittent period, found himself all at once, and without an evident cause, relieved of them; but he perceived at the same instant an excruciating pain a little above the elbow, and nearly about the middle of the thigh. He continued ever afterwards quiet free of all the symptoms of polypus; but they were succeeded by a large abscess in the parts in which he felt the pain. That in the arm disappeared gradually; but the other became so large as to occupy the whole of the under part of the thigh. The cure was effected by passing a seton through the whole length of the tumour; by the use of two dozen of Maderia wine, bark, and a calomel pill with opium, three times a day.

The audibleness of the palpitation of the heart in these cases, Dr. Chisholm remarks, may be considered an exaggeration; but in one instance, particularly, a gentleman (Mr. McSween), to whom the negroes belonged, heard distinctly the palpitation from an adjoining room.

Dr. Chisholm states that, at the onset of the epidemic, he knew not how to act until dissection showed him; and adds, that having no suspicion of the heart being the seat of the malady, he did not examine that organ in the first two bodies opened; but finding the brain and all other organs in a state of health, he opened the heart in the third body, and discovered what he considered might be the cause and seat of the disease. In the right ventricle of the heart he found an enormous polypus, which extended considerably into the pulmonary artery. On extracting it, the body which was contained in the ventricle, measured two inches in breadth. In the fourth body there was a large polypus in the right and left ventricle, besides one in the right auricle. The hearts of the fifth, six, and seventh were just similar; and in these five, except one, where the lungs were diseased, no other morbid appearance could be found.

It is interesting to remark, as an aid to our therapeutics, that Dr. Chisholm, after he had discovered what he considered to be the cause of death, viz., the concretions in the heart, changed his plan of treatment. On the very first appearance of symptoms, he took blood, and then freely salivated, with mercury. The results, he adds, were most happy; not one that was salivated died.

I can offer, on my own part, no such a narrative as that given by Dr. Chisholm; but I can very definitely refer to not fewer than six well marked cases in which death, taking place rapidly, with no other foreboding symptoms than a rigor and succeeding slight febrile heat, was traceable by the *post mortem* inquiry to deposit of fibrine in the right side of the heart; the deposit filling the auricle, to the exclusion of blood, and extending in the course of the circulation, from the auricle into the ventricle and pulmonary artery.

In other instances, as we have seen, the deposit of fibrine is subsequent to the development of one or other of those forms of local mischief to which we assign a specific name. The obvious features of such cases are distinguished by the presence of the local change, but the essential characters are the same in all examples, that is to say, the fibrine is abnormally increased. The increase may extend from the normal two and a half or three parts per thousand, to six, nine, or it has been said to twelve parts per thousand. This latter figure is possibly derived from an error in analysis, but the fact of a great increase is without a shadow of doubt; the result of the increase is also often equally striking. The excess of fibrine, beyond a limited extent, cannot remain in suspension in the blood, and the heart, under such conditions—if so plain but expressive a term be allowed me—*churns* out the fibrine, and becomes the receptacle of the separated mass.

I can give, at this moment, no explanation of the first cause of increase of fibrine in the cases I have named, except by entering upon the whole question of hypercausis and its origin; which task I would rather avoid, or at least retain. It will be more instructive, at this time, to pass to the subject of the symptoms of concretion, concerning which all is demonstrative.

[There is a class of cases in which fibrinous deposit in the heart is the sole cause of dissolution. Many examples of this are met with in cases of death from puerperal fever. There are other cases, as acute pneumonia, pleurisy, and, in fact, most sthenic inflammations, which the deposition of fibrine renders fatal, and which would not otherwise have proved so. In other cases the fibrinous deposit is a consequence of a condition in itself necessarily fatal, as phthisis and cholera.]

In all cases of the acute inflammatory type, whether slightly or broadly defined, the practitioner who is conversant with the risks of fibrinous deposition will be on his watch tower. He will remember the symptoms by which the existence of deposit is recognised; he will be guarded in his promises, and careful in his predictions. Most of all, he will be protected from being misled by those rapidly developed changes from severe local disease to suppression of local symptoms, which so often give to illusory hopes, and usher in the death.

Again, as I have said, the knowledge of the existence of a fibrinous concretion in the heart may prevent the committal of meddlesome, and, because useless, mischievous practice. In days when blood-letting had its full swing, when everybody was bled once in every disease, and once a day in inflammatory diseases; when even the convulsion of hemorrhage was met by a recall on the lancet; in those days there arose, amidst all the confusion, this persuasion, that to bleed late in inflammatory disease was bad practice. If I remember rightly, immortal Mackintosh himself, on every page of whose 'Practice of Physic' there is a gaping vein, admits this rule; and the reason of the rule is obvious enough. In the later stages of inflammation, the tendency to fibrinous deposition is increased. The effect of hemorrhage is to increase the volume of water in the blood, to distribute the fibrine solvent, and to encourage fibrinous deposition. When, therefore, late in the inflammatory state blood is drawn, the balance of the blood constituents, already disturbed, is disturbed the more, and all that is wanting to secure the deposition of fibrine is secured. The rule, therefore, as I say, originated that it was better in inflammatory disorders not to bleed late in the disorder; and this rule expanded now into wider pretensions, goes pretty nearly to the length of telling us that it is well to avoid bleeding at all stages; not because bleeding is not a grand remedy, but because it is too grand a remedy to be used without a definite knowledge of the time when to use it. I have, nevertheless, seen the rule about late bleeding in an inflammatory disease disobeyed even in our own time; and therefore I enforce it, and give the reason for its enforcement. In a case of acute pneumonia, which came under my observation some years ago, I was overruled to take blood. After the bleeding, the strong man fell like a shot bird. He fell into sleep, and he slept into death. A large fibrinous concretion blocked up the right cavities of his heart. This clot had possibly commenced to form before the venesection: afterwards its rapid increase was determined, and the death was hastened by the means intended for its prevention.

The remarks against the general abstraction of blood in the later stages of inflammatory disease tell with little less force against local blood-letting. Indeed, there is not unfrequently a greater danger in the local than in the general remedy, inasmuch as, owing to its supposed harmlessness, it is more fearlessly resorted to. "A few leeches can do no harm," is the common sentiment: it is an incorrect sentiment, nevertheless, and to be guarded against always, and with special care in cases of acute hyperinosis in the young. Speaking, indeed, of hyperinosis in children, I would, if my voice had any influence, raise it against the abstraction of blood altogether in such examples There is a form of acute pneumonia often met with in well-fed fat children. We put on leeches; the bitten surfaces bleed furiously; but the disease is not stopped. From such a patient, the more the blood taken, the greater the risk incurred; for, whenever the local indications of inflammatory mischief subside, the general indications of deposition are in the ascendant. "Breathless, pale, and bloodless," the end of the patient is at hand.

The same rule which tells against venesection extends to the administration of two classes of medicaments—purgatives and opiates. To reduce the amount of water in the blood, by draining it off through the alimentary canal, is but to increase relatively the amount of fibrine in a blood in which the fibrine is already absolutely increased. And so, once more, to paralyse the heart with opium, during the existence of acute hyperinosis, is to double the danger of deposition by bringing into play that feeble condition of the circulation which we have described as in itself immediately favourable to fibrinous separation.

In those states where the increase of fibrine is relative, or where there is tendency to deposition from sheer feebleness of the circulation, the rule against depressant measures is the more strongly to be enforced. I can add nothing further in this direction.

We will move from what may be called negative treatment to our third point—the lesson taught by our knowledge, such as it is, regarding a positive course of proceeding for the prevention of deposition. And here, I think, the first great truth which appears is that, to treat disorders marked by hyperinosis successfully, we must treat them by depressants at their early stages only, and must be prepared to move from the depressant system at much shorter notice than is commonly considered necessary. Before there is positively established in the blood an absolute increase of fibrine, I have no doubt that the conditions leading to such increase may be struck down by the use of rational means for the reduction of the force of the circulation; and so sometimes an acute inflammatory disease may be averted by free venesection. But, as it is rare that the practitioner is called to see disease in this early stage, blood-letting is dangerous to recommend as a general remedy. Whither, then, shall we turn to a remedy? We must turn, certainly, to those remedies which, while they reduce the force of the circulation, possess also a solvent effect on the blood. Is mercury such a remedy? I see no reason, either of a physiological or practical kind, to lead me to an affirmative on this question. In its physiological effects on animal organisms, mercury, carried to salivation, produces a peculiar disintegration of blood, but not a fluidity: in many of the cases of acute hyperinosis, in which after death I found large concretions, I trusted throughout to mercury, and gave it boldly but with no result. The old calomel and opium pill is, therefore, to my mind, a combination deserving its loss of caste. When such a pill does good, I suspect the opium should have the sole credit, if not a little more.

Is antimony deserving of more favour? I believe it is. In a large number of experiments, in which I placed animals under the influence of antimony, I found in every case that the effect of the substance was to maintain a fluid condition of the blood; while common experience confirms that the drug has a power of arresting the force of the circulation. The danger of antimony, indeed, is that it may kill by the very depression which it produces; and its value, to put the opposite, is in proportion to the sthenicity of the disease. Its value, consequently, is limited: add to which, the remedy may be put into the body very much quicker than it can be carried out; so that its administration in excess is a pretty sure way of securing a prolonged convalescence, should the hyperinosis even be checked, and the patient recover.

Salines, what of them and their uses in hyperinosis? In these, I opine, we have not only the oldest, but the best remedies. I speak specially of the vegetable salines. As I have often pointed out, it is a curious fact that the common vegetable acids, citric and acetic especially, act on the blood exactly as the alkalies; they maintain, that is to say, the fluidity of blood. In combination, therefore, these bodies have a rational intention in the treatment of all disorders attended by excess of fibrine, particularly when they are given with a free quantity of water. Given in full and frequent doses (and it signifies, probably, but little which acetate or citrate is chosen) they are, after all, the safest, and, according to our present knowledge, the surest medicines in the diseases under discussion. It is not easy to push them too far; they do not accumulate; they put a gentle check on the circulation; and their transformation in the economy does not apparently diminish the solvent effect produced on the blood by their administration.

The narcotic series of remedies, dangerous as they become in late stages, are invaluable in the early stages of inflammatory disorder. I do not now speak of opium alone, but of the volatile as well as of the stable narcotics. The whole class have the physiological property of arresting the oxidation of the tissues, and, as a result, of calming that excitement which is the preliminary and necessary step to hyperinosis. The more rapidly acting narcotic is therefore often the best; and if chloroform, as the most rapid of ordinary narcotics, were a practical remedy—a remedy which could be entrusted to other than medical hands for its administration—it were, I believe, the best antiphlogistic known. I have treated croup at its onset with chloroform by inhalation, with a success such as I have seen from no other plan of treatment. But the effects of the drug must be fully carried out; and, unfortunately for practical utility, the effects must also be sustained, often for many hours, and must be watched with an attention which none but an experienced mind can give. Opium, from being more convenient in its application, has the advantage of chloroform as a presentable remedy; but, as it is so much the less rapid in its action, and so much the more permanent in its effects when these are elicited, it is in a physiological sense so much the less valuable as an active remedy against deposition.

External applications in the shape of counterirritants, blisters and sinapisms, whatever may be their influence on the local lesion, can have no direct influence in preventing deposition. If cantharidin applied to the cutaneous surface be absorbed, it may have an effect on the blood; but whether that is for good or for evil it is impossible to say. I do not make these remarks on counterirritants in opposition to them as local remedies for local affections, but simply to indicate that they are not to be considered as of any avail in meeting the more general mischief present in the inflammatory condition.

Presuming that the practitioner is under suspicion in a case of acute hyperinosis or in a case of an asthenic kind, that deposition of fibrine is recurrent, what course shall he pursue? I know of but one: it is to give up at once all idea of carrying depletion further, and to throw in ammonia freely but with discretion. The carbonate of ammonia is the remedy usually given; but the liquid ammonia is the most direct and most satisfactory remedy: well diluted, it may be given in doses of five, ten, and by increasing it, twenty drops; and it should be given until it shows itself demonstrably and easily in the breath on the application of the hydrochloric acid rod.

It may be given at the same time that quina or the decoction of bark is given; but in most cases I suspect it forms alone the natural remedy. It is a chemical remedy; that is to say, applied for definite intentions.

But, while I thus recommend ammonia as a remedial measure which has to me proved of most value in the cases under description, and as a remedy which I have often brought into use in the early stages of acute disease, and always with advantage, I warn you that, in prescribing it, its effects must be carefully watched. Carried to an extreme, it produces the very opposite condition to hyperinosis; and then it is no longer a remedy—it is a poison. In one case, recorded in my essay on the *Coagulation of the Blood*, in which five grains of bicarbonate of ammonia were given every two hours, the remedy acted, I fear, as a poison to a certain extent. The patient was sinking, and in some measure comatose, when the administration began; all the excreting processes being much diminished. After death, the odour of ammonia in the blood was most distinct. The blood was dark and thick, but uncoagulated. I collected half an ounce from the right side of the heart, and exposed it to the air for three days: it underwent feeble coagulation, but its true plastic quality was clearly destroyed. The corpuscles, as is common in such blood, were disintegrated, and modified in character; there were some stellate, others oval, others many-sided, others collapsed; but not one was of the natural appearance.

Let me, then, renew this precaution relating to the action of ammonia, and extend it to the other alkalies. Indeed, the effects of potass and soda, carried to the extent of producing the same changes, are more serious than those produced by ammonia; for the volatile alkali being volatile, can escape more quickly from the system than the fixed alkalies, after its withdrawal: it holds, in fact, as a physical agent administrable to man, the same relation to soda and potassa as chloroform holds to opium or belladonna.

In those extreme cases where the fact of absolute deposition is too clearly demonstrated, what, then, shall be done? Alas! I can at this moment point out no satisfactory solution of this question. The day will come when the veil shall fall from our or our successors' vision; but now we can but grope our way, and pray for the light. I for one know however, the difficulties in the way; and this knowledge, perchance, may be useful to some one in the future. I will therefore sketch them forth as matter for thought, and aids to future work.

It will occur to you, that the proceeding to be first considered is the removal of the fibrinous deposit by causing its dissolution. This is a definite proposition: the difficulties in the way are equally definite. In the first place, we possess no agent having a sufficiently rapid solvent action on separated fibrine. When fibrine is as yet in solution in the blood, it is the easiest matter in the world to keep it fluid; the addition of a very small quantity of alkali, say two parts of alkali to a thousand of blood, is sufficient for this purpose. But, when once fibrine has separated, its resolution is a difficult matter. True, it is not less soluble than before; but time is destroyed in the process. If, for instance, a solution be made containing so much as four grains of alkali to the thousand grains of a blood containing two grains of fibrine, a period of at least six hours must elapse before such fibrine would be redissolved. But such an amount of alkali thrown into the mass of blood as would bring that fluid altogether to the alkalinity thus required, would be in itself impracticable, because fatal. Under such alkalinity, the blood-corpuscles would be entirely dissolved.

Again, when deposit is the fact, the administration of alkalies by the mouth is in no way a direct means of effecting solution of the obstructing body. Medicines introduced in this way, granting that they are taken up into the blood at all, which is in truth doubtful, are but slowly carried through the heart, and are brought into contact with the concreted mass at a single point. From such limited exposure we can expect no result.

The administration of ammonia by inhalation in combination with the vapour of water, seems at first a reasonable suggestion. I have tried it both in disease and in experimental work. It fails. Ammonia is not absorbed easily by the lungs; and, as absorbed, it is never present in the blood, passing stroke by stroke through the heart, so as to produce any effect on a deposit of fibrine in this organ.

The injection of alkaline solution into the veins may seem a reasonable project. It is worthy of trial in extreme cases, but I fear it would not succeed. If solution of clot could be determined by this process, I am afraid there would also be determined universal dissolution of the walls of the blood corpuscles; and an animal without corpuscles in his blood is no longer alive. The first microscopist of blood little knew upon what he had dropped when he beheld these little invisibles. If we could or should at any time light on a chemical body which will dissolve fibrine on immediate contact, without destroying the blood-cells or the coats of the vessels, then we should have a remedy on which we might set our hopes. The difficulties in which the question is involved, by means of this antithesis of argument, will be readily understood.

The observations I have made above are, however, intended to bear entirely on cases of a kind where death is threatened within minutes or hours: in that series of cases where the symptoms extend over a long period, such cases as that recorded by Dr. Sayer and Mr. McNab, a course of treatment by a fixed alkali might possibly be productive of real good. Such treatment is based on philosophical and sound argument.

Returning to extreme cases: Would it be possible, when the concretion is on the right side of the heart, to remove it by operation? I will relate an experiment bearing on this point. In one case, where a cat died in oxygen gas, I took the animal out of the box immediately after she seemed to cease breathing, laid bare the right external jugular vein low down in the neck, pushed through it a crotchet-needle, passed that into the heart, and brought up to the point of insertion a separated mass of fibrine, which had before obviously filled the auricular cavity. In cases of dying in the human subject, where all is hopeless, I see no harm in doing a similar operation; the diagnosis of concretion on the right side being clear.

Before I conclude, I have to touch for a moment on a special question which has been specially asked of me. It is this. What are the conditions leading to hyperinosis? To this question I give frankly the answer, that it is at present unanswerable. To approach it even requires a new and extended inquiry into the whole subject of the oxygenation of animals—a point to which I may at some future day be enabled to call the attention of the profession, with a knowledge yet to be learned, and aspirations yet to be realized!—*British Med. Journal, Feb.* 11, *and April* 7, 1860, *pp.* 101, 259.

26.—ON THE EXISTENCE OF A FIBRINOUS CLOT IN THE HEART IN SOME CASES OF CROUP.

By HENRY SMITH, Esq., Caroline Street, Bedford Square, London.

[Some years ago a controversy took place between Dr. Richardson and Mr. Henry Smith, respecting the causes of death in croup. Dr.

Richardson asserting that in some cases, attended with peculiar symptoms, the cause of death is to be found in the existence of a fibrinous clot in the heart, formed several hours prior to dissolution. When death is brought about by obstruction to the breathing alone, the symptoms are widely different. These opinions Mr. Smith considered erroneous. Now, however, writing upon the same subject, he observes :]

Further inquiry on this matter, and observation on both the living and the dead, have convinced me that Dr. Richardson's views deserve serious attention. As on the former occasion I considered them incorrect, and opposed them as well as I was able ; so now, from a sense of justice towards that gentleman, I am glad to state my present conviction, that there are certain cases of croup in which the symptoms are more referrible to an embarrassed state of the heart than to a mere obstruction in the windpipe. These symptoms consist of great dyspnœa, pallid face and lips, cold extremities, and very feeble pulse; whereas, turgescence and lividity of the face, with blueness of the lips, accompanied with extreme dyspnœa, mark the insuperable obstruction in the trachea. In the former cases I am now disposed to agree with Dr. Richardson, that tracheotomy will not save life, as there is every probability that a coagulum has formed in the heart, whilst in the latter experience teaches that an artificial opening in the windpipe may save life.

It is not the mere reflection on Dr. Richardson's views that has brought about an alteration in my mind respecting this point; but a careful examination, both on the living and the dead, has led me to believe that that gentleman is in a measure, at least, correct; and in order to be brief, I will just mention the details of one of the last cases of croup, where I was able to make a post-mortem examination :—

I was called to a child, aged 6, who was suffering severely from croup, and had been ill three days. The breathing was most laborious, the child making violent efforts to get air, and not moving the chest at all; the face was, however, very pale, and the lips were not blue. The pulse was extremely rapid and small. My opinion was sought regarding tracheotomy; I did not think it a favourable case. The operation was not done; and the child died in an hour.

I opened the body with the express view of ascertaining whether there was a clot in the heart; for the child presented just those symptoms expressed by Dr. Richardson. Surely enough, on opening the heart, there was seen extending between the right auricle and ventricle a firm deposit, and there was another fibrinous clot at the commencement of the aorta. The lungs were healthy, but the larynx was lined with a thick deposit, which very much narrowed the cavity ; a thinner membrane lined the trachea. Now, it struck me at once, that had tracheotomy been performed in this case, the existence of the coagulum in the heart would have prevented recovery had the obstruction in the windpipe been overcome; and I am now of opinion that some of the cases previously operated upon by myself, and presenting just the same symptoms as existed here, were instances where a fibrinous clot would have been found in the heart had it been looked for.

In another instance, where I was recently called to perform tracheotomy, the symptoms had come on within only a few hours. The child, aged 5, was lying in bed perfectly insensible, breathing with extreme labour; notwithstanding this, the pulse was pretty good, and the extremities not cold. I refused to perform the operation, and the child died in an hour and-a-half. On examination there was not any coagulum in the heart whatever; but the lungs were excessively congested, and the upper part of the larynx and trachea were lined with an organised cast.

Without venturing to go so far as Dr. Richardson has gone, I cannot help thinking that his views regarding the existence of a fibrinous clot in the heart in certain cases of croup demand serious attention; and, as I once strongly opposed them, I am now happy to admit that I am, in a measure at least, converted to his side.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 617.

27.—ON THE INFLUENCE OF THE NERVES ON THE ACTION OF THE HEART.

By John Cockle, M.D., Physician to the Royal Free Hospital, &c.

The exact influence of the par vagum upon the heart is still a vexed question, although the accurate experiments of Edward Weber have never yet been challenged. He clearly proved that electro-galvanization of these nerves retarded the heart's beat, and prolonged the period of diastole; while, on the other hand, their section was followed by marked acceleration of the pulsations; so that, as Ludwig has remarked, the eighth nerve holds the balance of the heart's contractions. It is, in other words, the moderator nerve of the heart. But while admitting these experiments, what different conclusions may be drawn, if the views of Dr. Brown-Séquard prove to be correct! This physiologist maintains that the par vagum is the true vasomotor nerve of the coronary arteries, and that its irritation causes contraction of these vessels, and thus, by preventing the passage of the blood into the cardiac tissue, diminishes the frequency of, and eventually suspends, the heart's beat; while, on the other hand, their section, by allowing the coronary arteries to become over-filled with blood, excites the heart to preternatural pulsation. Still there are residual phenomena that do not admit of such a physiological solution. Take, for example, the palpitating heart of a chlorotic girl, beating for days at the rate of 120 or more per minute; then compare this condition with that of some cases of marked fatty degeneration of the cardiac muscle, where the heart's beat is lowered to 20 per minute; and who is to say, with the authority that should entitle him to credit, what part is in such cases due to the nerves, on the one hand, and to the impoverished blood and to the degenerated tissue, on the other? Look at the dilemma we should fall into, for example, if the action of the heart, in anæmia, were considered as dependent upon the motor nerves. In accordance with experiment, if the par vagum were irritated the standard of pulsation should be lowered, not raised; or, if the accelerated pulsation were referred to the central ganglia, why then the par vagum does not in reality hold the balance.

The explanation of the mediate influence of the sympathetic nerve upon the heart is still more obscure. It is familiar knowledge that tænia in the intestine can greatly derange the heart's rhythm and rate of speed, but the exact *quo modo* is still open to conjecture.—*Lancet, Dec.* 17, 1859, *p.* 610.

28.—ON CERTAIN POINTS OF PHYSICAL DIAGNOSIS IN MITRAL VALVE DISEASE.

By John Cockle, M.D., F.L.S., Physician to the Royal Free Hospital.

[The writer, before passing on to the subject-matter more immediately in question, premises some interesting observations on the physiology of the heart's action, which, however, do not bear so directly on practice as to be inserted here. Dr. Cockle then continues—]

The diseases of insufficiency, perhaps, as a rule, entail less serious mischief than those originating in obstruction. Regurgitation into the left ventricle through the faulty aortic valves may, as is well known, last for years, with scarcely a subjective symptom, so long as the ventricle maintains its contractility unimpaired. So, in like manner, with moderate regurgitation through the mitral valve. This is, perhaps, of all diseases of the heart, when slight in degree, one of the most easily tolerated; it may also last for years without necessarily entailing any marked degree either of hypertrophy or dilatation of the heart.

The left auricle, with respect to the regurgitant blood, acts the part of a safety reservoir, just as the tricuspid valve permits blood to regurgitate into the auricle during temporary distension of the right ventricle, and, in the cases named, effectually prevents stress upon the lesser circulation. Its thick and highly elastic endocardium powerfully supports the muscular tone, and averts the danger of impaired contractility. Hence, in these cases, beyond the knowledge possibly of a prior attack of rheumatic fever, the systolic murmur at the apex is the only physical sign which we have to depend upon as marking the change. That the disease is kept within these moderate bounds, we may indirectly know by the auscultation of the lung. So long as the breath-sound remains pure or but slightly exaggerated, we may conclude that the disease does not exhibit any tendency to increase. This is the surest criterion that I can indicate for such purpose. The mere existence of systolic murmur, beyond revealing the existence of structural change, is here at its minimum of value. Indeed, usually, the louder the murmur the less the actual amount of mischief inflicted.

To these cases, when chronic, medical treatment, in its strict sense, is quite inapplicable. We cannot restore the integrity of a valve which has been injured by an endocardial disease of long standing; and, so long as the general health remains good, we have simply to enforce the avoidance of all causes likely to induce embarrassment of the systemic and pulmonic circulation.

These slightly dilated hearts, however, are, it is well known, much more prone to attacks of palpitation than those in the opposite condition of simple hypertrophy. I would here momentarily allude to the distinction between palpitation of organic and inorganic origin, and observe that, now and then, perhaps, the surest diagnostic sign between the two conditions is the absence of marked dyspnœa during the paroxysm of inorganic, and its presence in cases of organic palpitation. I have already stated that this stage may endure for years unless some causes, physical or mental, occur to impair the general vigour and induce impoverishment of the blood. Irritative excitement is then induced, and now it is that the first signs of impaired contractility of the chamber present themselves.

This failing muscular power of the heart is still further taxed by blood insufficiently stimulating. It is in this early stage of mischief that special treatment is so applicable—I mean the union of digitalis and iron. I know no treatment to compare with this combination. But for its special virtue, digitalis must be given in small (four or five minim) doses, the great indications being to lessen irritability, to promote regularity of rhythm and speed, and thus to ensure the complete contraction of the chamber, and, as a consequence, to impart stability and volume to the pulse. By this treatment not only are such objects achieved, but the physical signs are ameliorated, and the intensity of murmur greatly reduced. I believe that the external use of belladonna, in the form of either plaster or ointment, must act in an analogous manner. But should the dose of digitalis, however, be larger, and given to the extent of prolonging the period of diastole, we at once lay the foundation for the supervention of that condition it is our sole object to antagonize—dilatation of the chamber. There is another possible mode by which digitalis exercises its diuretic qualities. In marked regurgitant disease, diminished secretion of urine is a common symptom, and is probably a direct result of the diminished amount of blood supplied to the kidney. Now, when digitalis acts as a diuretic, it may be that, by permitting the ventricle to empty itself more completely, the aorta supplies the kidney with more blood, and consequently more urine is secreted. At least this is the opinion of Prof. Niemayer, of Greifswald.

But, in more exaggerated cases of regurgitation, the great question arises—how is such a condition physically to be recognised? Does a simple and permanent systolic murmur, harsh or soft, with its maximum intensity at or near the left nipple, reveal the inadequacy of the valves to close the orifice, or may such murmur simply indicate roughness of the ventricular face of one or both curtains, or some abnormal position of a tendinous cord? Skoda has stated, *ex cathedrâ*, that an absolutely intensified accent of the pulmonic sigmoid valves, in consequence of the impediment offered to the free passage of blood through the lungs, is the pathognomonic sign of this stage of regurgitation, and I need hardly mention the weight of authority such a statement bears. Without pretending to deny that such accent, at times, unquestionably exists, it does appear to me (and I mention it with the deference due to a great name) that there are some objections to the general reception of this theory. For example, I believe the following propositions susceptible of proof:—

1st. That the second sound of the heart, in health, varies in point of character; that is to say, it is sometimes most loudly intoned to the right, at other times to the left, of the sternum.

2nd. That the second sound stands in no fixed relation to the energy of the ventricular systole. Dr. Stokes has recorded many cases of fever, in which, the walls of the heart being softened, the first sound was annihilated, while the second retained all its intensity.

3rd. That the second sound stands in no fixed relation to the resiliency of the arterial wall; this sound being loudly heard in some cases of extensive atheroma, in which the elasticity of the aorta is more or less destroyed.

4th. That an obstruction at the aortic mouth would, on this theory, naturally tend equally to throw stress upon the second sound of the pulmonary artery.

5th. Where considerable regurgitation exists, the diminished stream propelled into the aorta would, theoretically, tend to weaken the closure sound of its sigmoid valves, and thus leave the sound of those of the pulmonary artery not absolutely, but only relatively intensified.

6th. That in many cases the systolic mitral murmur is so intense as to occupy an entire revolution of the heart, and entirely to mask the closure sound of the pulmonic sigmoid valves.

7th. That in some cases of auriculo-ventricular contraction, the cardiac sounds are so indeterminate, and the rythm so disturbed, that nothing like particular accent can be discovered.

Lastly. Many changes of lung tissue—such as cirrhosis, contraction from phthisis—may permit the pulmonary artery to come into more immediate contact with the chest wall, and thus give, relatively, intensified accent. Induration of the left lung border may, also, become a source of reinforcement of the sound of the pulmonary artery.

So that, if these propositions are valid, accent upon the second sound of the pulmonary artery cannot always serve as a means of differential diagnosis in the diseases of the mitral valve.

The next point to determine is, are there any other physical phenomena which can at times subserve differential diagnosis, and even enable us to measure approximatively the amount of regurgitation? I believe that there are two.

First. The heart, in such cases, being under the influence of irritative excitement, with a varying amount of dilated hypertrophy, acts more energetically than it would do in a normal state, though transmitting a diminished quantity of blood into the systemic vessels, in consequence of the back current into the auricle. But the subclavian and carotid arteries, in their recoil, thus forcibly and suddenly injected, closing on the diminished stream, give rise to the phenomena of collapsing and unfilled arteries, precisely as we see in cases of spanæmic palpitation, as, also, that following considerable loss of blood: the closure of the sigmoid valves, audible, at least, at the top of the sternum and in the carotids, proclaims their integrity, while the smallness and softness of the radial pulse exclude the notion of mere uncomplicated hypertrophy of the left ventricle only. Thus, in a sense, this condition not only indicates actual regurgitation, but, to some extent, estimstes the amount. This condition has been, in two cases, carefully noted during life, and the condition of simple inadequacy of the mitral valve verified after death. This sign, however, requires, of course, more extended verification.

The second sign of occasional importance, and immediately conditioned upon the preceding, relates to the reduplication of the second sounds of the heart, having, of course, their maximum intensity at the mid sternum.

The following is the theory I would suggest respecting the occurrence. The aorta, receiving a diminished supply of blood, in consequence of the regurgitant stream into the left auricle, fails of its accustomed stimulus of distension, and, as a possible consequence, lags behind the pulmonary artery in its recoil. The closure sound of its sigmoid valves would then, in point of time, be subsequent to the recoil of those of the pulmonary artery, and produce the phenomenon of reduplication. This sign, also, might both characterize the disease, and, within certain limits, measure the amount of regurgitating blood. There are, however, certain difficulties occasionally presenting themselves in this particular—namely, when the second sounds are trebled, or even quadrupled. Where reduplication occurs with the first sound, it is, I believe, nearly always the result of a decided rasp murmur. A great authority in this country, Dr. C. J. B. Williams, as also an observer in France, Dr. Charcelay, have supposed that the reduplication of the first sound originates in a want of synchronety of the ventricles. If this explanation be correct, it appears to me that there should, from an almost inevitable necessity, be reduplication of the sigmoid valve sounds also; but this result does not of a certainty always follow. Another physical sign in regurgitation through the mitral valve has also been mentioned by Skoda—viz., a murmur audible in the pulmonary artery; and he explains the production of such murmur by organic change in the end-arterial membrane of the vessel. Now, it is certain, that in mitral valve regurgitant disease, not only does systolic murmur exist at the apex, but it is, at times, heard loudly two or three inches above and around the left mamma, occasionally completely masking the second sound; it is also audible at the corresponding site posteriorly, and in the course of the great vessels. Now in such cases we may assume a double origin of murmur—that not only is vibration engendered at the faulty valve, but that it is reinforced, or even regenerated, by the refluent stream from the ventricle meeting the comparatively stiller layer in the auricle.

In other cases, when, with weak murmur at the apex, murmur—it may be of different pitch—is after a time heard in the vessels, it, in all probability, arises from the distended auricles pressing upon the aorta and pulmonary artery respectively, just as gentle pressure upon any large artery with the stethoscope produces temporary murmur at will. At all events, whatever be the explanation of this murmur in the great vessels, it is safer to assume a mechanical cause, similar to the one just named, in preference to the supposition that organic change exists in these vessels. In the young, at least, we see that double valvular disease is not the rule; inasmuch as two different pathologic causes are often necessary for its production. The aortic valves are mostly affected by the atheroma of advancing years, while endocarditis has a peculiar affinity for the curtains, cords, and fibrous zone of the mitral valve.

Regurgitant and obstructive diseases of the mitral valve differ from aortic regurgitant and obstructive diseases in their effect upon the chambers of the heart, but all agree in one great particular—viz., the gradually increasing tendency to under-supply of the arterial, and over-supply of the venous systems. This latter result in the systemic system arises from the diminished *vis à tergo* consequent upon the insufficient supply of arterial blood, and by such mechanism, these affections lead eventually to impaired contractility of the right side of the heart, producing those congestions, hemorrhages, and effusions which form the common terminus of the diseases of the central organ of the circulation.

In conclusion, I would select one case from the series, both for the purpose of illustrating the peculiarity of the physical phenomena, and of signalizing an error into which I was betrayed with regard to the differential diagnosis.

Charles T——, aged fifty-three, by trade a shoemaker, was admitted a patient of the City Dispensary in 1856, with the following symptoms, which had been gradually increasing for the three preceding years:—Palpitation, at times in severe paroxysms of long duration: considerable dyspnœa on exertion; cough, occasionally combined with sero-sanguineous expectoration. The extremities were cold, swollen, and livid; face of livid pallor, and tumid; lips leaden; the pulse in both radials, extremely small and most irregular; the carotid arteries throbbing and collapsing; the jugular veins swollen and undulating—though emptying on full inspiration, but soon distended from below when pressure was applied above the vein. The heart's apex was felt to the left of the left nipple. The percussion area of dulness was increased in both the vertical and transverse directions. The impulse was extensive, weak, and irregular, but without fremitus, extending from the apex to the epigastrium. On auscultation, the heart's rythm was markedly irregular, and both sounds were replaced, so far as could be made out, by an extremely slight and harsh murmur, of osseous (?) character, if I may use such a term. This double sound, apparently loudest at the neighbourhood of the left nipple and towards the left axilla, was still audible, in a less degree, over the middle and lower half of the chest, right and left. Slight sibilating and mucous râles were heard over the lungs generally. His history showed that he had never suffered from rheumatic fever.

The above is a fair paraphrase of his symptoms, from notes taken on several occasions while he was under treatment. He was most carefully watched for nearly three years, in consequence of the symptoms noted, and the physical signs and murmurs heard, supposing them to indicate beyond doubt, auriculo-ventricular contraction of the left orifice, with insufficient tricupsid valves; and that, sooner or later, an opportunity might be afforded of substantiating the cause of the existence of a double murmur in such a case, and so setting at rest a vexed question: for I need hardly say that some of the ablest observers in France have most emphatically denied that a double murmur ever occurred in contraction of the left orifice. During the further course of the disease, frequently recurring dropsy, slight in extent, took place, and he at last died from increasing complications.

The post-mortem examination was made by Dr. Robinson, who saw the patient several times during the closing period of his illness. The following was the condition of the heart:—Great and general dilatation of all the cavities and vessels of the heart, excepting the aorta. The walls of the cavities were very soft, but this was, in all probability, the result of post-mortem change. The right auriculo-ventricular orifice was immensely dilated, admitting with ease the five fingers; the tricuspid valves being inadequate to the closure of the orifice, obviously the result of secondary insufficiency. The left auriculo-ventricular orifice was also greatly dilated. The larger or aortic curtain of the mitral valve was unusually dense and much increased in size; the smaller curtain was altogether *rudimentary*, not exceeding in size the thumb-nail. The chordæ tendineæ were extremely short and slender, so as scarcely to permit the valve to leave the wall of the heart. Considering the absence of rheumatic fever and change about the valve, beyond its abortive condition, it was probable that it was a congenital malformation.

Now, I do not pretend to offer any explanation of the physical signs observed in this singular case, but I consider the following inference fairly warrantable from the post-mortem results. In the great majority of cases of heart disease, we can state that valvular lesion exists; beyond this, we may in many even predicate the particular orifice affected; but that occasionally our present lights are insufficient to guide us to a correct differential diagnosis between the obstructive and rergurgitant diseases of the auriculo-ventricular orifices.—*Lancet, Dec.* 24, 1859, *p.* 636.

29.—ON THE TREATMENT OF PERICARDITIS.

By W. O. Markham, M.D., Physician to St. Mary's Hospital, London.

Extended observation has proved that in the treatment of acute rheumatism there is no remedy which has such a powerfully beneficial influence over the disease as the salts of potash, when administered in sufficiently large doses. The proof of this assertion is to be found in the results of our daily practice; and in the concurrent—I may say, universal—testimony of competent observers in different countries.* The remedy, indeed, is entitled to be considered as something of a specific in this disease, so constantly are the expected results found to follow its proper use. From one to two scruples, according to the age of the patient, of the bicarbonate of potass are to be given for the purpose every second hour, as prescribed by Dr. Garrod. A marked amelioration of the symptoms is generally —though, I admit, not invariably—observed to follow its administration in the course of about twenty-four or thirty-six hours; in fact, whenever the urine becomes alkaline.

Now, inasmuch as we are bound, in all reason, to believe, that the *materies morbi* which produces the rheumatic arthritis is identical with the *materies morbi* which produces the rheumatic pericarditis, we should, to follow out a rational treatment, anticipate that this remedy would be also efficacious in preventing, or arresting the progress of, the pericardial inflammation. The fact is not, for obvious reasons, easy of demonstration, but observation most certainly tends to show the correctness of the conclusion. The authority of many competent observers might be cited in support of the opinion thus expressed by Dr. Garrod: "Besides the influence on the duration of the articular affection which has been alluded to, I cannot help thinking that an effect is likewise produced on the cardiac disease, to a very considerable and important extent. In no case did the affection of the heart ensue after the patient had been more than forty-eight hours under the influence of the medicine; and it has appeared to me, that even when present on admission into the hospital, or coming on within a short period, its progress was powerfully checked by the treatment, and prevented from producing the terrible mischief which, when uncontrolled, it so frequently induces: this I should be inclined to ascribe to the altered condition of the blood, and especially of that portion giving rise to fibrinous deposits on the peri- or endocardium." *(loc. cit.)*

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* The following are a few of the authorities who may be referred to on this point:—Dr. Basham gives from one to three ounces of nitrate of potash in four pints of water during the twenty-four hours. (Med. Chir. Soc. Trans., 1848). Gendrin also gives nitrate of potash. (Med. Gaz., 1848). Dr. Golding Bird used the acetate of potash in doses of half an ounce in the twenty-four hours much diluted. Dr. Swett administers one drachm of the tartrate of soda and potash every two or three hours, until the urine becomes alkaline. (New York Med. Times, 1854). Dr. Garrod's method is to give two scruples of the bicarbonate of potash every second hour, night and day, in a wineglassful of water, "until the joint-affection has ceased for three or four days." (Med. Chir. Trans., 1855). To this excellent paper, I would especially call the attention of those desiring particulars in this matter.

My own observation fully bears out Dr. Garrod's statement; and, necessarily, therefore, leads me to the conclusion that we possess no remedy more efficacious than this, in the treatment of acute rheumatic pericarditis, *quoad* the diathetic character of the disease. Its action is, we must suppose, (from its manifest influence over the arthritis), the neutralising of that poisonous element, whatever it be, which occasions the local inflammations—the arthritis and pericarditis; arresting the progress of the inflammations, when they have already seized upon parts, or anticipating and preventing the seizure.

[Where severe arthritic affections coexist with the pericarditis, it is absolutely necessary to tranquillize the nervous system, and opium must be given, if necessary, freely. Restlessness, sleeplessness, pain, and suffering, act very injuriously upon the case. The opium may be given in grain doses every hour for hours together, and it will not be found to "lock up" the secretions or contract the pupil.]

During the whole treatment of pericarditis, the secretions should be duly attended to. The bowels should be regularly relieved, and the action of the skin and kidneys maintained. Frequent purging, however, must be avoided; for the partial exposure to cold, which is its necessary consequence, and the extreme pain and suffering which movement is apt to occasion in these cases (especially when the co-existing inflammation of the joints is severe), sometimes more than counterbalance, by the nervous excitement and general disturbance they produce, the good which the purging might be supposed otherwise to bring about, as a process of elimination.

The *general* treatment, then, of rheumatic pericarditis is in the main—at least, during its first periods—the treatment of acute rheumatism. But the local inflammation demands especial consideration. In the commencement of the inflammation, the local abstraction of blood, proportioned to the degree of local pain, the strength of the patient, and the period of the disease, is generally of service. Should there be signs of severe pulmonary and cardiac congestions, then small venesections will be required for their relief. Warm fomentations or poultices should be constantly applied over the præcordial region. This method of bringing warmth to the inflamed part is of very great service; it gives much relief to the patient; and is worthy of more attention than is generally given to it in this country. The beneficial effects of blisters have been doubted by some observers; but I have so often seen them followed by mitigation of the local pain, and apparently by diminution also of the pericardial effusion, that I do not hesitate to use them, when the first acute periods of the inflammation have passed away. They do not prevent the application of poultices or fomentations.

At this period of the disease, another most important object, arising out of relation of the inflammation to the heart, presents itself to our consideration.

In every case of percarditis, indeed, whether rheumatic or non-rheumatic, and at all periods of the inflammation, we should carefully watch for signs of the heart's failing powers. Weakness in this disease, is apt to manifest itself suddenly. This warns us to be cautious, in the first instance, that we do not injure the patient by depletion; and it also warns us not to delay too long the use of stimuli. There are cases, especially of the non-rheumatic class, in which stimuli are requisite from the very beginning of the attack; the condition of the system, under which the percarditis has arisen, demanding their use.

This attention to the condition of the heart in pericarditis is a point upon which Dr. Stokes has especially insisted; and there can be no doubt, most wisely. He is "convinced that patients are often lost from want of stimulation at the proper time." The following are the particular signs, which he has given, as indicative of a weakened condition of the organ: a feeble, intermittent and irregular pulse, especially when it has not had these characters from the first; turgescence of the jugular veins; change in the heart's sounds, particularly feebleness of the first sound; pallor, coldness of skin, œdema, and faintings.

Here I would again impress upon the student the facts: that, inflammation is a sign of weakness not of strength; that in every inflammation where exudation has taken place, there is a process of absorption and reparation to be gone through, if the parts affected are to regain their integrity; and that for the proper furtherance of this process, vital force is absolutely necessary. Sinking or weakness of the vital powers should, therefore, be carefully anticipated in all inflamations; but especially so in pericarditis, because, here the paralysing effects of the inflammation fall directly upon the central organ of the circulation. Light and easily digestible nourishment—milk and weak broths—should therefore be assiduously administered during the earliest periods of the inflammation; and stimuli, when the acute period has ceased, and the process of absorption has commenced, or whenever the signs of enfeebled circulation begin to show themselves.

Observation, indeed, certainly seems to prove, that rheumatic pericarditis is an inflammation which rather attacks those of weak than of strong constitutions; that it is more common in the delicate and young, than in vigorous persons at the prime or middle periods of life; and that the degree of inflammation—that is, the general febrile reaction and the local exudation—is also greater in them than in the strong. The disease is sometimes especially severe in young females, and in such cases, the violence of the inflammation is shown by the abundance and rapidity of the pericardial exudation.

From all which considerations, this corollary necessarily results, viz., that in the treatment of pericarditis, the administration of our remedies must be measured by the general condition and the idiosyncrasy of the individual attacked, rather than by the intensity or other peculiarity of the local inflammation. And herein—in seizing upon the special points of the individual case, and in modifying the remedies to meet their requirements—lies that peculiar practical tact which distinguishes the skilful physician.—*British Med. Journal, Dec.* 10, 1859, *p.* 996.

DISEASES OF THE ORGANS OF RESPIRATION.

30.—ON THE USE OF ANTIMONY IN TUBERCULAR PNEUMONIA.

By W. T. Gairdner, M.D., Physician to the Royal Infirmary, Edinburgh.

[In a case of Pneumonia probably tubercular in its origin, in which the patient was almost in extremis, (pulse almost inappreciable and skin cold, upper lobe of left lung condensed, and breathing amphoric) Dr. Gairdner, with the best results, gave small doses of antimony, along with diffusible stimulants; he gives antimony in these cases notwithstanding the weakness and exhaustion of the patient. Of course the recovery was only partial, it could scarcely be otherwise considering the state of the lung. The dose of antimony]

Should rarely exceed one-tenth or even one-twelfth of a grain to begin with in such cases, sometimes even less. But, on the other hand, it is sometimes so well borne, that I have given half a grain and even one grain doses to patients who were so weak as to be utterly unable to raise themselves in bed; and this without the slightest disagreeable effect of any kind. In general I regard the ordinary physiological effects of antimony as *quite opposed to its therapeutic action;* and whenever they occur, I make it a rule either to suspend the remedy or to diminish the dose,—believing it to be, on the whole, much safer to forego the possible advantage of the antimonial medication, than to run the risk of superinducing the least degree of poisonous action.—*Edinburgh Med. Journal, April* 1860, *p.* 953.

31.—ON SOME CONSIDERATIONS IN THE TREATMENT OF PHTHISIS.

By EDWARD SMITH, M.D., LL.B., Assistant Physician to the Hospital for Consumption, Brompton.

[A normal alkalinity of the blood is absolutely essential to the proper performance of the combustive process, whilst acidity is necessary to the formative process, (hence the muscular fluid, and, indeed, the fluid of every tissue has an acid reaction), therefore,]

Excessive waste in the system will occur from two different causes; viz., from excess of acid and excess of alkali; and each, perhaps, is equally injurious. Now, we know that acids and alkalies constitute the great part—indeed, on reflection, we might say almost all—of the remedies in phthisis, as, indeed, in most diseases; and by practice we attain to a sort of undefined notion as to the cases in which they are respectively applicable. The difficulty is to determine with accuracy these respective states. In reference to the acid state of the blood, I think we may always affirm that unusual acidity of the excretions (not due to local states of disease of the excreting organs) indicates an excess of free acid in the blood; and, in phthisis, I am disposed to consider the state of the skin-excretion as the best indication of this state of things. If, in such cases, I find that the perspiration has a sour odour (and the number of such cases is legion), the administration of a combination of the carbonates of soda and potass, say fifteen or twenty grains of each combined, or of the acetate of ammonia, thrice a day, is followed by the happiest results; and the measure of the effect is still the state of the perspiration. This immediately tends to increase the oxygenating process, but it does not of necessity remove that state of assimilation by which an undue amount of acid is thrown into the blood (assuming that, on inquiry, the excess of acid is owing to that circumstance); and hence a reduction in the starchy and saccharine food, and the invigoration of the system by change of air, &c., must be added. The amount of perspiration does not guide us in this matter; for in many cases, with excess of acid, there is profuse sweating; and with profuse sweating we are accustomed to associate acid in our treatment, yet, in such cases, the proper administration of the alkali tends to remove this excess of perspiration. I have recently had a case of early phthisis under my care, in which there was a constant excessive elimination of acid by the skin, and which, without other symptoms than excessive and general exhaustion and wasting, did not progress satisfactorily during three or four months. Under the influence of the combination of potass and soda, he had a very profuse sweating, accompanied by an exceedingly acid and acrid odour; and obtained immediate and almost marvellous relief. The following is a short account of this case:

J. E., a young single man, aged 20, engaged in a grocer's shop in the country, had complained for three months of bad appetite, with brown tongue, foul breath, and slight emaciation. There was slight hæmoptysis occasionally, with slight dyspnœa. There was scarcely any cough, but the throat had been sore from the commencement of his disease. The general expression of the symptoms was nervous exhaustion and muscular debility. The chest presented no signs of disease, except the slightest shade of dulness on percussion, and lessened respiration on the left side.

On August 21st, he began to take iron, with a mineral acid and quina. Cod-liver oil was added on August 28th. In September, he continued in the same state, but with a little increase of cough and debility. In October, the cough continued, but he felt a little general improvement. There was also a little pain in the left breast. His state was becoming more anæmic: and the iron, being intermitted for a time, was renewed in November. During all this period he took out-of-door exercise, used salt and water ablutions; and was directed to take milk and other nutritive kinds of food at short intervals.

Thus three months passed away, without any visible improvement; and the progress of the lung-disease appeared imminent. At the end of November, however, he had a most profuse sweating during four nights, in frosty weather, accompanied by an exceedingly sour and acrid odour, which appeared to be the crisis of his condition; for, under the influence of bicarbonate of potass and gentian, he improved most rapidly, and the increase of perspiration, with its acid state, ceased in December. Throughout January he continued to improve, and became again cheerful, active, and strong; and the cod-liver oil being again prescribed at the beginning of February (it having disagreed with him in the early part of his attack), caused so much improvement in a fortnight, that he was led to look upon it as the principal means of his restoration to health. I cannot but think that, if the alkaline treatment had been begun earlier, we should have been spared some anxiety. The case shows well the peculiar condition of blood under discussion, the efficacy of the remedy, and the different action of the oil under the two conditions.

It is in this class of cases that I believe fats to be the most beneficial, if their administration is preceded and accompanied by the administration of these alkalies. There can be no doubt that the administration of starch tends largely to increase the acidity of the blood; and there is reason to believe that the action of fats (the other common hydrocarbonaceous material) tends in the contrary direction—a belief based upon the physiological action of fat upon the respiration, the influence of fat over oxygenation, its beneficial action in cold climates, and its influence under the conditions now being discussed. Hence I think that the state of the perspiration offers an important guide for us in the administration of alkalies and fats. A similar remark may also be made in reference to the action of numerous nitrogenous substances over the respiratory functions; for my experiments have proved that they have this action, and also that it is an indirect one by influencing the rapidity of the oxygenating process, and not a direct one by supplying carbon. As I have recently discussed these subjects elsewhere ('Philosophical Transactions', 1859; 'Philosophical Magazine', December 1859; 'Dublin Medical Quarterly Journal', Feb. 1860), I shall not enter into them here, except in reference to one which, both from its power and its universal employment, should be better understood; viz., tea. We do not take tea without other food, except it be after the principal meal of the day, when the system is still replete with food; that is to say, we do not take it at the period when the blood is unusually alkaline, but when it abounds in free acids. Its action is rapid, and so powerful that a maximum increase of one-fourth or one-fifth of the whole quantity of carbonic acid exhaled is obtained in less than half an hour; and it has so accumulative an action, that twenty-five grains, taken every quarter of an hour, gave an excess of two hundred grains of carbonic acid within about one hour—a quantity of carbon much greater than it contained. Hence it has a most powerful indirect influence in promoting oxygenation, and is especially fitted for the condition in which there is a disposition to excess of free acid. I am desirous to press this upon your attention; for I believe that, in such cases, tea is our best and most certain medicine. I have also found it of great value in cases of night perspiration, when a large breakfast-cup is taken cold in the night—of greater value far than any other remedy, except the removal of too much bedding, or warmth, or damp. The detection of the exudation of the blood, in which there is an excess of free alkali, is rather by negative than positive indications; but when there is wasting, languor, defective appetite and assimilation, soft and perspiring skin, and no excess of acid in the urine or perspiration, we have the indications of its existence. This is a condition which prevails in hot weather and climates, and one on which the administration of phosphoric as well as the vegetable acids is of great avail. Fat is commonly less indicated, and such remedies as tea may be injurious by increasing the waste. Cold bathing with salt and water is of great value in both classes of cases, but especially the latter; and the preparations of iron in cases of anæmia are then also much more indicated, since the excess of alkali promotes their solution, and permits a larger amount to be collected and retained in the blood. Milk is a food which I advise to be taken at frequent intervals in the day, and especially in the early morning, and is in a degree applicable to both these states of the blood; since, whilst it has a free alkali and albumen on the one hand, it has fat and phosphates on the other. I believe, however, that skimmed milk, or milk made sour, is much more fitted than new milk for cases in which there is no excess of acid; and it will be found to be better borne by the patients. Milk has a direct action, by supplying plastic and heat-forming materials; and an indirect one by its casein, in promoting assimilation. I also take the opportunity of saying that we have been too much led away from the use of milk and cream in supplying fat to the system, by the habit of administering separated oils; and I think it is easily capable of proof that the combination of the oil in a molecular state with the other components of the milk renders it far more assimilable than any separated fat can be, and especially a fat administered alone; that is, apart from the nitrogenous substances which I have proved to be necessary for its assimilation.

From the point of view in which I have been accustomed to regard phthisis for some years, I have considered that voluntary deep inspirations were well fitted as adjuncts to treatment; and this, both because with greater amount of air inspired even voluntarily there is increase in the chemical changes which follow, and because with a lessened degree of distension and vital action proceeding in the air-cells there must be a progressive tendency to a degradation of their structure, and, as I believe, to the filling up of their cavities by epithelium, or the earliest state of tubercle in these organs. The advantage of this method has for advocates no less authority than Lehmann, who, on physiological grounds, commends it in cases of deficient respiration, in bronchitis; and Piorry, who has recently affirmed, as may be seen by an extract in the Medical Times of January 5th, 1859, that by this means he has been able to cause a large amount of expansion in cases where *deposits* even had heretofore prevented it. Dr. Mühry, a German writer, who has recently treated of climate, seasons, and elevation, over the degrees of expansion of the lungs, has supported the view that expansion of the air-cells is a desideratum of great importance. Hence, in addition to my own affirmation that in numerous cases of early phthisis I have been able to increase the existing diminished vital capacity of the lungs, I think there is evidence to convince the minds of those who regarded my plan with distrust, that there is good reason for its adoption, and that it is not too mechanical to be excluded from physiological and even pathological employment. The increase in the function of the lungs, and the prevention of that tendency to deficient expansion of the air-cells, both alike tend to prevent the further deposition of tubercle; and if, from various reasons, they should fail, the present unsatisfactory result of all kinds of treatment leaves us nothing to regret. I cannot but attach great importance to both of these objects; viz., the increase in the vital actions of the vesicles, and their due expansion and expansibility; and both, in fact, are accomplished in our ordinary directions for violent exercise for the maintenance of health.

I cannot avoid the expression of my conviction that in many cases, owing to the deficiency of vital action, whilst the appetite for food is not materially lessened, the system becomes overburdened with ill-digested material, tending to the accumulation of carbon in the blood, and materially impeding the respiratory process. In such cases, diminution of food, with increase in the action of the liver and bowels, do very much to relieve the oppression of the breathing, and to aid in the work of increasing the expansibility of the air-vesicles. In such cases, alcohols are directly contraindicated; and, indeed, in almost all cases of phthisis (excepting, perhaps, those in which the depressed action of the heart is a marked feature), the advantage of the employment of alcohols is apart from, and in spite of, the alcohol. —*British Med. Journal, Dec.* 3, 1859, *p.* 982.

32.—ON THE USE OF THE HYPOPHOSPHITES OF SODA AND OF LIME IN THE TREATMENT OF PHTHISIS.

By Richard Quain, M.D.

[The plan of treatment of phthisis, by phosphorus, recommended by Dr. Francis Churchill, has been tried by many physicians, and has been reported fully by Dr. Cotton (Retrospect, vol. xxxvii., p. 84). Dr. Quain has lately, in consequence of the perusal of Dr. Churchill's treatise, been engaged in re-examining the subject, and has embodied the results of this enquiry in the following article.]

It will, perhaps, be fair to say, in the first instance, that Dr. Churchill states that he was led to adopt the use of the hypophosphites in consequence of his belief that the tuberculous diathesis depended on some disturbance in the process of sanguification; that this disturbance, which affected the inorganic and not the organic elements, was due to a deficiency and not to an excess of some one or other of these elements. He argued with himself, that it could not be the sulphur, the iron, the chlorides, or the alkalies, for these substances were daily used as remedies, without any real effect on the disease. Eliminating, then, the elements first-named, he concluded that the failure was in phosphorus as a constituent of the body.

It should here be noticed that these propositions of the author can only be regarded as theoretical speculations, inasmuch as they are unsupported by either chemical or physiological observations.

By a similar course of reasoning, but one more in accordance with physiological facts, Dr. Churchill arrived at the conclusion, that phosphorus, the missing element, could be best supplied by the adminissration of this body in its lowest state of oxidation, as it was thereby given in a form more capable of assimilation. With that view, he administered the hypophosphites of soda and of lime, which he declared to be prophylactic, and to be curative in every stage of the disease. He says: "I *know* that they will prove, not only as sure a remedy in consumption as quinine is in intermittent fever, but also as effectual a preservative as vaccination in small-pox."

Encouraged by statements like this, and by a lengthened catalogue of the phenomena of improved health which it was said resulted from the use of these remedies in Dr. Churchill's hands, I determined on giving them a fair trial in a certain number of cases. They were, therefore, administerrd in twenty-two cases, taken without selection from amongst the ordinary in-patients of the Brompton Hospital. Of this number (twenty-two), twelve were males, and ten were females.

The Stage of the Disease.—Two cases were in the first, ten in the second, and ten in the third stage of phthisis.

The Dose of the Remedy.—Dr. Churchill recommends ten to thirty grains as the dose, of either the hypophosphite of soda or of lime, daily, in any simple fluid. The dose to be increased, until the general symptoms disappear. In some cases, he prefers the one salt to the other. For example, he thinks that the salt of lime checks the expectoration, and thereby increases the cough: whilst the salt of soda is less energetic in its action. I met with nothing confirmatory of this impression. The dose given to the patients at Brompton was, in the first instance, ten grains, three times a-day, except in the case of a child, when only five were given. The disease progressing, or being stationary, or the effects of the remedy being *nil*, the dose was gradually increased. Thus, in four cases, it was increased to a drachm three times a-day; in ten cases, the dose reached two scruples or more; in eight, the dose remained under half a drachm. It will thus be seen that the remedy was given freely. In no case, let me add, was there any appearance of the troublesome symptoms indicated by Dr. Churchill as following large doses.

The Duration of the Treatment.—One case was under treatment for six months, one for four months, six for three months, nine for two months, five for one month. During this lengthened course of treatment, I looked anxiously, but in vain, for those marked physiological effects described by Dr. Churchill. There were no evidence of the "improved powers of innervation;" "the hair and nails did not grow more rapidly;" there was no "appearance of plethora or of fulness;" the patients did not describe "an unaccustomed sensation of feeling better and stronger after a few doses of the remedy." Nay, I would say that there was nothing more felt by the patient nor noticeable by the physician than if so many grains of carbonate of soda or prepared chalk had been taken.

The Results.—To return, then, to the more immediate object for which these agents were administered, viz., to ascertain their value in the case of consumption—I have to state, that of the twenty-two cases, six were more or less improved while under treatment. Of these six, three were improved in but a slight degree, and only for a short time; in three the improvement was marked, but in one only of the latter has the improvement been permanent; of the two other cases, one continued using the hypophosphite for three months after leaving the hospital, during which time she grew gradually weaker, and finally died; the other, a man, after leaving the hospital, continued the treatment for some time, but gradually grew worse, and is now dying.

All the other sixteen cases steadily lost ground whilst using the hypophosphites in the hospital. Happily, in six of these cases, the treatment by hypophosphite was suspended, and the usual treatment by cod-liver oil, tonics, &c., being substituted, a decided improvement in each was the result.

[The opinion of one who signs himself "R. C. H." is entirely opposed to that of Dr. Quain. He says :]

I have now prescribed the hypophosphites in about twenty cases. They were in various stages of the disease, chiefly in the first and second stages; two were in the third stage. One case has taken the remedy twelve months; all the others, about six months. They are still under treatment. So far from no physiological effect being produced, I have generally found in each case the following effects:—A great increase of appetite; increase of animal heat (in some cases, not in all); a *marked diminution* of the expectoration in every case; the patients also expressing a feeling of improvement in their breathing power—that they could fill their chest better with air. On making an examination of the chest with the stethoscope, I have found the moist clicks to diminish in number; in one instance they disappeared entirely, and were replaced by vesicular breathing, perhaps somewhat harsher than natural: this was an instance where the disease was of limited extent in the upper part of the right lung, but nevertheless a well-marked case. The hypophosphites appear to act as respiratory excitants; and I should certainly be disappointed upon testing their breathing power with the spirometer, if the volume of air inspired at each act of inspiration was not increased. I have not a spirometer, and, therefore, have not made the experiment; but, judging from the respiration becoming almost puerile in some portions of the lungs, when before its administration the respiratory murmur was so feeble and indistinct as to be scarcely heard, and also from the loss of breathlessness experienced by the patients themselves, I certainly should, from a consideration of the above phenomena, expect to find that such would be the case. Physicians connected with public institutions could easily settle this point.—In October last I gave a young man, æt. 20, five grains three times a day. He had all the symptoms of phthisis; had diarrhœa; was drenched with morning perspirations; had no appetite; and his loss of flesh and strength was so rapid, that it threatened to be a case of galloping consumption—there was unmistakable evidence of crude tubercle in the lungs. When he had taken the hypophosphites for a month, his appetite became *voracious;* he gained weight at the rate of three pounds a week for three successive weeks, and felt so strong and well that he resumed his work as a factory operative, and has continued at it until the present time. I saw him again last Saturday (February 25th): he is now losing flesh, and has a dry cough, with partial aphonia—no night sweats. I advised him to rest, and resume the hypophosphites; with what results, time will show. No other remedial agent was used in this case, except nutritious diet.—A sister, æt. 25, of the last patient, has persevered with the hypophosphites for twelve months. She has had chronic phthisis for five years; it had been kept in abeyance by cod-liver oil, until twelve months ago, when, from some unexplained cause, her digestive organs would not tolerate it any longer, when she rapidly declined in health. I then gave the hypophosphites. The improvement was more marked and permanent than in her brother's case. I saw her again on February 25th, when she expressed herself as being quite well—never better in her life. She had a robust appearance, and was very stout. All the general symptoms have disappeared. I had not an opportunity of examining the chest. So far from no effect being produced, I am in the habit of prescribing hypophosphites in any exhausted condition of the system where loss of appetite and debility are the most prominent symptoms. The following case briefly illustrates their utility in a case of strumous dyspepsia, attended with debility:—On June 14th, 1859, the treatment with hypophosphites began: his weight was six score seventeen pounds; no appetite, and weak. June 20th, seven score one pound. August 13th, seven score five pounds, his appetite and strength keeping pace with his increasing weight.—On January 14th, 1860, I gave the hypophosphites to a farmer, æt. 43, who had taken various tonics for a month with no improvement to his appetite and strength. On making an examination of his chest, I found evidence of a slight deposit in the upper part of the left lung. He had gradually lost flesh, and had been troubled with dyspeptic symptoms for six months. On the administration of the hypophosphites, his appetite gradually returned, and he began to gain weight, having gained four pounds in a fortnight. Surely here is evidence of some physiological power? The largest dose I have given has been half a drachm a day, in doses of ten grains each. Very few of my patients can take more than five grains three times a day, without producing headache and unpleasant feelings of fulness about the head. In one case, epistaxis came on. All my patients took plenty of out-door exercise when the weather permitted. No very satisfactory improvement took place in a less period than three weeks. I should be afraid to give a drachm-dose three times a day.—To recapitulate, I claim for the hypophosphites a power of stimulating the appetite for food—which has taken place in every case; a power of rapidly diminishing the expectoration in all cases under my observation, and, in some, completely arresting it; a power of increasing the expansion of the chest; and, theoretically, of being respiratory excitants; and they present us, at the same time, with "very efficient blood-generating agents." My experience does not enable me to say they cure phthisis; but that they retard its progress, particularly that of *softening*, I am quite convinced; and that they are a useful auxiliary in the treatment of phthisis, whatever other power they possess, has been established to my satisfaction.

[Dr. Dickson, of Jersey, writes thus:]

In the month of October last, while in Paris, I visited Dr. Churchill's Dispensary, and being satisfied from what I saw that there was something in his treatment of consumption by the use of the hypophosphites, on my return to this place I put the treatment in practice, and have found it so beneficial in the majority of the cases so treated that I am induced to persevere. I have tried it in about thirty cases, *in every stage of the disease*, and in the majority—say *two-thirds*—the improvement has been very marked. My experience has proved that it is decidedly beneficial in the earlier stages of the disease, although one very material effect of the medicines (viz., their power of checking *night sweats*) makes them very useful in all stages. What convinced me most in the cases I saw under Dr. Churchill's care, was the fact that those who presented themselves at the Dispensary were generally those of the lower classes of society, who came there labouring under phthisis (as proved by auscultation and percussion), and who then returned to their daily avocations, bad ventilation, bad drainage, bad food (half the time perhaps nearly starved), and obliged to work for that food—therefore I reasoned, if these cases improve there must be something in the treatment. Since my return from Paris, I have requested several medical friends to try the treatment on their patients, and they have mentioned to me that the *result was most extraordinary* They have given the treatment a much fairer trial than Dr. Cotton and the Brompton M.D.'s have ever done. Time alone can show how far the improvement may be permanent. My experience, however, satisfies me that in the hypophosphites we have a powerful agent, and that, on the score of humanity alone, they ought to have a fair and impartial trial. I may mention that the result of the treatment on a near relative of mine, who has been for some months under Dr. Churchill's care, is as satisfactory as it is gratifying.—*Lancet, March* 17, 1860, *p.* 267, *and Med. Circular, March* 14, 1860, *p.* 177.

33.—*Pneumonia: Production of Ægophony.*—At a recent meeting of the Association of the King and Queen's College of Physicians in Ireland, Dr. Law read a paper on a case of pneumonia which he had had under his care in Sir Patrick Dun's Hospital. The inflammation had occupied a very large portion of the left lung, occurring in a young man of 23 years of age. The case had been treated by the administration of minute doses of calomel frequently repeated. Dr. Law is often able to produce salivation with three and a half or four grains of calomel, by giving but the twelfth of a grain each dose, and repeating it every hour. He considers that the system becomes more easily affected by the mercury if it be combined with small doses of tartar emetic. In the present case the disease was completely got under in five days. In the course of his observations, Dr. Law combated the opinion of Laennec as to the manner in which ægophony was produced. He did not think that great author correct in attributing the peculiar sound referred to, to the action of the voice in causing a vibration in the fluid effused in pleuritis, but rather believed it to be due to the vibration in the compressed lung, and looked on ægophony and bronchophony as produced by different degrees of compression. He held that there might be ægophony without any effusion into the cavity of the pleura.—*Dublin Med. Press.—British Med. Journal, Jan.* 21, 1860, *p.*

34.—SIMULATION OF PNEUMOTHORAX BY AN ENORMOUS CAVITY IN THE LUNG.

(Under the care of Dr. James Jones.)

The following case is interesting, as an example of the simulation of the physical signs of pneumothorax by an unusually large tubercular cavity. The size of the cavity, and the almost complete limitation of disease to one lung, are also features of interest:—

M. A. T., aged 26, was admitted an out-patient of the Infirmary for Consumption, Feb. 1859, under the care of Dr. James Jones. The early history of her case, so far as could be learned, presented all the symptoms of slow phthisis. At the time of her admission she was much wasted, pale and weak. The left side of the chest was very resonant throughout, except over the posterior part of the lower lobe of the lung where, for the space of four inches, there was well-marked dulness on percussion. Amphoric resonance was heard all over the side, but loudest between the fourth and sixth ribs, where, also, metallic tinkling was frequently audible. Pectoriloquy was painfully strong. There was no displacement of the heart, no distension of the intercostal spaces, nor was any change in the site of the dulness on percussion produced by alteration of posture. About two months before death, œdema set in in the feet, and soon became general. The lips were not livid until the last week of life. Dyspnœa increased concurrently with the œdema, and loud mucous *rales* were audible all through the right lung. There was abundant expectoration of a mixed kind, partly muco-pus and partly viscid mucus. From the setting in of œdema the urine became loaded with albumen. Consciousness remained perfect to the last. She died on Dec. 19.

Necropsy.—The left lung was found completely excavated from the apex downwards, to within about three inches from the base. The interior of this huge cavity was smooth, the lower part being imperfectly sacculated. Its external wall was from one to two lines in thickness, and strongly adherent to the costal pleura. It contained about half-a-pint of very fluid pus. Several bronchi of the third and fourth magnitudes opened into it: some above and some below the level of the fluid. The right lung was highly congested, exuding on incision an abundance of frothy fluid. It contained a sprinkling of granular gray tubercles. A tubercular cavity the size of a kidney-bean, was found in the apex. The liver was healthy and free from tubercle. The heart also was healthy. The kidneys were not examined.

The points of interest of this case, as pointed out by Dr. James Jones, are,—

Firstly. The almost complete absence of old-standing disease in the right lung at a period of so advanced disease of the left.

Secondly. The enormous size of the cavity which excavated nearly the whole of the lung. The continuance of life under such circumstances depended to a great extent on the capabilities of the other lung in performing the functions of respiration to the extent necessary for existence.

Thirdly. The almost perfect simulation of the physical signs of pneumothorax resulting from the large size and the smoothness of the interior of the cavity, the thinness of its external walls, and their close adhesion to the thoracic parietes. Generally when amphoric resonance and the metallic clink are produced by tubercular cavities, they are heard only in the upper part of the chest—in a word, over the cavity—but here they were audible all over the chest, and most clearly between the fourth and sixth rib, as is usually the case in pneumothorax. A splashing sound was heard low down in the chest when the patient coughed. No respiratory murmur was audible in any part of the left side, the portions of lung which were left unconsumed by the disease being too much solidified to admit the entrance of air into their structure. The absence of any displacement of the heart, or of distension of the intercostal spaces, could not much assist the diagnosis, as their presence or absence in pneumothorax must depend on the greater or less freedom of communication, through the bronchi, between the air in the pleural cavity and the external atmosphere.

In the investigation of cases of this kind, the absence of suddenness in the manifestation of the physical signs will assist the diagnosis. A tubercular cavity is formed with comparative slowness, and the stethoscopic signs gradually develop themselves; but pneumothorax arising from the formation of an opening between a vomica and the cavity of the pleura, is sudden: to-day there is no amphoric sound, to-morrow there is. The advanced period at which these cases frequently present themselves at the hospitals and dispensaries unfortunately, however, deprive us of this source of information.

The various amounts of resonance to the stroke in different parts of the chest, depending on the usual inequality of the thickness of the external walls of the cavity, will also afford some assistance.

The quantity of fluid, also, contained in the cavity is seldom so great as to cause a marked change in the seat of dulness on percussion by alteration of posture. Besides which, the irregular shape or sacculated condition of the bottom of the cavity, where the progress of excavation is most recent and imperfect, prevents its fluid contents from flowing freely, whereas, in pneumothorax any fluid present immediately changes its seat, in accordance with the posture of the patient.—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 166.

35.—EPISTAXIS ARRESTED BY INJECTION OF PERCHLORIDE OF IRON.

By Dr. FOUNTAIN DAVENPORT, Iowa, U.S.

I am induced to publish the following case from reading the report of "Death from Epitaxis," occurring in the practice of Dr. Triplett, of Virginia.

About two years ago I was summoned in haste to see a young man who was reported to be in a dying condition from loss of blood. The bleeding had continued uninterruptedly for about thirty hours, escaping constantly from the nostrils, and frequently thrown out in clots from the posterior nares.

He had been attended by a German physician, who had not succeeded in arresting the hemorrhage, and before my arrival he had abandoned the case, from motives which I need not here mention.

I found him in a frightful condition, his face, hands, linen, and much of the bed-clothing, and the floor, being covered with blood. In one corner of the miserable apartment, where many people were crowded, was a pile of rags, and towels, saturated with blood.

He was so impoverished that he could not support himself in an upright position, and the extreme pallor of his skin and colourless lips indicated plainly that he had lost a large quantity of blood.

I immediately plugged the nostrils, anteriorly and posteriorly, in the usual way. I supposed this would arrest the hemorrhage, but I was mistaken. Very soon the blood escaped through the plugs in each direction, and the flow returned as profuse as before. I then removed the plugs, and rolling up two quite large pieces of fine, dry sponge, I introduced them as before, and so firmly, that I thought it would be impossible for a drop of blood to escape. I then left him, and returning in an hour after I was astonished to find the blood escaping as freely as ever. I again removed the plugs, and washing out the nostrils by injections of cold water, I pressed into each a roll of *tannin*, made into a soft mass with a little water and glycerine; I packed the nostrils full with this, but it did no good. I then introduced the plugs a third time, using compressed sponge, and forcing them in so firmly, that I feared I might have great difficulty in removing them. I then had ice applied constantly to either side of the nares, and kept the patient perfectly quiet. This answered the purpose for about half an hour, and then the bleeding returned as bad as ever. By this time the patient experienced the alarming symptoms of excessive loss of blood—ringing in the ears, occasional blindness, &c. The case looked desperate to me, while the patient, and even some of his friends, protested against further effort, as useless and cruel. I saw that it must be checked very soon, or it would surely be fatal. I removed the plugs with some difficulty, and washing out the nares, I passed into each a piece of nitrate of silver, about a quarter of an inch in length, carrying them back with the forceps about four inches. I also introduced a curved injecting instrument, perforated towards its extremity with a number of small openings, and forcibly injected a strong solution of the same caustic. This did no good.

It now occurred to me that the perchloride of iron might answer the purpose. I soon procured it, and after washing out the nostrils as before, I injected a quantity of the undiluted perchloride into each nostril. This immediately checked the bleeding, and proved an effectual remedy.

Twenty-four hours after this the bleeding again returned, when I repeated the injections, and with the same happy effect.

There was no recurrence of hemorrhage after this, and the patient soon recovered, so as to go about. but for quite a long time felt the effects of the loss of so much blood, which had been so excessive as to leave him but a very slender hold upon life. I know not in what other way it could have been saved ; and the publication of this may be the means of rescuing others in similar circumstances.—*Medical Circular, March* 14, 1860, *p.* 170.

36.—*Injection of Sulphate of Atropine in the Track of the Pneumogastric Nerve in Asthma.*—The last Paris novelty consists in this treatment, by M. Courty, of the paroxysms of asthma in a case which had resisted a great variety of medicinal agents. He injected along the track of the pneumogastric nerve, on the inner side of the sterno-cleidomastoïdeus and on a level with the thyroid cartilage six drops of a solution of the sulphate of atropine, which produced vertigo, dilatation of pupil, and other symptoms of narcotisation. Next day the injection was repeated on the other side, and thrown in more deeply, with the effect of producing still greater, though not alarming narcotism. A third injection, two days after the last, completed the treatment—the asthma having gradually diminished, and now, four days after the first injection, entirely ceased. The patient (a lady, aged 54) continued quite well two months after the treatment had been put into force.—*Moniteur des Sciences.—Med. Times and Gazette, Nov.* 26, 1859, *p.* 535.

37.—ON TRACHEOTOMY IN CROUP.

By James Spence, Esq., F.R.C.S.E., Surgeon to the Edinburgh Royal Infirmary.

[The operation of tracheotomy is often spoken of as one perfectly simple and easy; but when a patient is struggling in the agonies of suffocation its performance is by no means so simple as generally described.

Success depends a good deal upon attention to little minutiæ in its performance.]

There ought to be no attempt at rapidity or brilliancy in this operation; every step should be methodically gone about. First, as regards the position of the infant, the shoulders should be well raised, the head bent moderately backwards and supported by an assistant, who should pass one arm under the pillow behind the neck, so as it were to project or support it forwards, whilst with the other hand he keeps the head fixed with the chin in the middle line as a guide to the surgeon; the arms and legs may be controlled by a small sheet or large towel pinned round the body, the arms placed by the side; and when thus secured, one person can control the movements: care should be taken to see that the teeth are not firmly closed, but that the mouth be partially open and the nostrils kept free.

The external incision should begin over the cricoid cartilage, and extend downwards exactly in the middle line for at least two inches. In general, two large veins, one on either side of the mesial line and close to it, are seen when the skin is divided; the fascial texture between these, corresponding to the intermuscular line is readily divided by the point of the knife, and then these veins can be easily drawn aside. Sometimes one crosses the line to join the other, and is embarrassing, and may even require to be divided after being secured by ligatures. But the point most to be attended to is caution after separating the tracheal muscles. If the finger be placed deep in the wound at this stage, the trachea is felt pretty distinctly, and may seem so distinct as to warrant the surgeon entering the bistoury to divide the rings; but I hold this ought not to be done, because not only may there be the substance of the thymus gland flattened under the finger, but deeper, and passing from that gland to the thyroid, there exist numerous vessels which would bleed profusely. I have more than once pointed out this to the gentlemen assisting me at my operations. After separating the lobes of the thymus gland, even after these vessels and the thymus are pushed aside, I clear the rings of the trachea with the knife from loose cellular tissue, so that there may be no obstruction to the entrance of the silver tube when the opening is made in the trachea. On the tube being introduced, the head should be bent slightly forwards.

The form of the tube itself is important: it ought to be of such a size as will admit air freely without over-distending the trachea, and always double; the inner tube fitting pretty close, but not secured to the outer, either by spring or other fastening, so that it may be easily removed for cleaning, or expelled by the efforts of coughing, if obstructed by mucus.

In the after treatment, when there is much bronchial effusion, the use of emetics is very beneficial; but under no circumstances would I now resort to antimony, for its effects are most dangerous, as diminishing the expulsive power, and depressing the patient, inducing sinking. I am glad that my opinion on this head is strengthened by the high authority and great experience of M. Trousseau. In all respects, I find ipecacuan answer better as an emetic, without depressing or leading to dysenteric purging, whilst it induces moderate diaphoresis and allays the febrile condition. M. Trousseau is very decidedly opposed to the use of blisters in these cases, on account of the sloughing which follows. But in this respect I cannot so fully concur; for though I would be far from advocating the use of the ordinary fly-blister, still I have found the application of the liquid blister highly useful in some cases of bronchial complication, and when carefully attended to, I have never seen any bad results.

In regard to diet, in many cases it is necessary to give beef-tea and wine from the first; but in general I prefer for the first day or two a nutrient non-stimulating diet, such as milk and farinaceous food, and afterwards gradually giving animal food; but in respect to this we must be guided by the condition of each individual patient.—*Edinb. Med. Journal, Feb.* 1860, *p.* 698.

38.—ON THE MERCURIAL TREATMENT OF CROUP IN THE RURAL DISTRICTS.

By C. Fergusson, Esq., High Hesket, Carlisle.

[It is too much the practice to record the experience of large hospitals and towns, and overlook the experiences of country practitioners. Disease presents very different characters in large towns to what it does in country places. The following communication is one of much interest.]

The treatment was first suggested by one of the Hamiltons, I believe the younger. My father would give to an infant of six months a grain and a half of calomel every hour till it purges freely; to a year-old child two grains, and to one two years old sometimes even four grains, until you get the bowels well acted on, or it vomits repeatedly. He has always noticed that the stools are of a greenish colour, something like chopped spinach; and immediately consequent upon this discharge an alleviation of the symptoms follows. When that is observed, the dose must be repeated less frequently, perhaps only once in two hours for some time, then still seldomer, and, lastly, gradually abandoned altogether. Should the child be much weakened by its use, or from other causes, stimulants must be given freely. He says it is wonderful how much calomel children can take *in a short time* without affecting the bowels or purging violently afterwards. Sometimes he has known fifty, sixty, and even a hundred grains to have been given before the disease succumbed, or any material effect was produced. Salivation in this way cannot be produced, give it to any extent you like, *in children.* Of course he used occasionally, as the case indicated, auxiliary treatment of different kinds, such as emetics and the warm bath, both of which he is extremely fond of.

These suggestions, you know, are the result of experience in a *purely country practice*, where the children are strong and robust, and so different from those in a densely-populated town; and in carrying out such a plan of treatment you must be guided by that difference in constitution and stamina, and modify it accordingly. For instance, in strong children, he used to leech preparatory to the mercury. I suppose you would hardly do that in any case in London. However, that does not affect the virtue of the mercurial treatment, he thinks, in the least. By the way, he would commence it at the most advanced stage—in fact, as long as he could get it swallowed. He is fond of an emetic of pulv. antim. tart. at the commencement, if the patient can bear it, and occasionally applies sinapisms over the root of the neck.—*Lancet, March* 10, 1860, *p.* 241.

39.—ON THE ANATOMY OF THE LUNG.

By A. T. H. Waters, M.R.C.P.L., Liverpool, and James Newton Heale, M.D., Winchester.

[Dr. Waters and Dr. Heale are at issue upon the anatomy of the bronchial arteries. The following extracts from the correspondence on the subject express the views of each on the subject. Dr. Waters observes that the opinions generally entertained by anatomists may be summed up as follows:]

That the bronchial arteries, being distributed to the bronchial tubes, areolar tissue, &c., return their blood *partly* by the bronchial veins and *partly* by the pulmonary veins; the bronchial veins being supposed to accompany the arteries *within* the lungs. Such was the view I entertained when I began my researches; but observation of a large number of human and other mammalian lungs convinced me that no bronchial veins exist *within* the lungs, and that the arteries pour their contents, except so far as they supply the structures about the root of the lung, into the pulmonary veins.

This was to me the discovery of a *new fact*, alike opposed to the opinion of Dr. Sharpey and other anatomists; and although I subsequently found that a similar view had been held by Reisseisen, sixty years ago, yet that author gave no proof in support of his opinion, and it remained without being refuted or confirmed. I do not, therefore, claim, with reference to the fact, priority of discovery; but I venture to think I have given the proofs which were wanting to establish it, and I believe subsequent researches will confirm them.

I shall not dwell on any physiological or pathological importance the fact may have, but simply refer to another instance in the body where an artery terminates in a somewhat similar way—viz., in the liver.

In my Essay I have endeavoured to do justice to previous writers, and I have pointed out what I believe to be the cause of the mistake —for such I must be allowed to consider it—into which Dr. Heale has fallen; and to the observations I have made, I beg leave to refer that gentleman. His views are of so novel a character, that they naturally arrested my attention, and I was particularly cautious not to reject them until repeated examination and experiment had satisfied me of their unsoundness.

[Dr. Heale writes :]

Dr. Waters seems to take up the ordinarily received opinion that the bronchial mucous membrane is supplied from the (so-called) bronchial arteries; that these anastomose with the pulmonary system of vessels, and that the bronchial arteries are the vessels by which the bronchial mucus is furnished.

1st. Were this indeed the fact, it would inevitably happen, in all cases of chronic bronchitis and bronchorrhœa, that the bronchial arteries would become prodigiously enlarged. I deny that such takes place.

2nd. Were that the true distribution of the bronchial arteries, whenever these vessels were fully injected it must necessarily occur that the bronchial membrane itself would also be injected. I assert, on the contrary, that it is impossible to inject the bronchial membrane from the bronchial artery; that the thing has *never* been done, and *never* can be done. There are many who will SAY that they have accomplished this; but I challenge them to produce a single instance (proper precautions which I shall enumerate having been taken) in which this has ever been fairly done. Should even one such case be duly established, I will assent that my views shall be considered as utterly refuted. I make it a *sine qua non*, however, that the bronchial arteries shall be injected from the aorta, otherwise it is quite easy to select a small twig of a pulmonary vein, and inject it under the supposition that it is a branch of the bronchial artery. This fact will account for many of the blunders which have been made on the subject. Moreover, I stipulate that the subject selected for injection shall not be a child that has died of croup, nor a patient who has suffered from extensive ulceration of the air-passages, since in such cases it might be found that the true lining membrane had been partially replaced by false membrane, and this might possibly be injected from the bronchial arteries; but even in such a case, the distribution of the bloodvessels would be strikingly different from that which prevails in the true bronchial membrane.

When the bronchial artery has been minutely injected, if the bronchial tubes be laid open, certain injected bloodvessels will be obscurely visible, and at the first sight one might, perhaps, be tempted to declare triumphantly that the bronchial mucous membrane was injected, but if a careful inspection be made, it will be found that the mucous membrane may be wholly raised, leaving the injected vessels behind, and if this raised membrane be then clarified by acetic acid, spread out on glass and compressed, a few hair-like vessels, sparsely scattered through its tissue, will be seen, by the aid of an object-glass, with a focal distance of half an inch. This fact shows that the cellular tissue merely of the raised membrane derived its supply from this source, but that the bronchial plexus and the function which this discharges are wholly distinct from the bronchial artery, and its branches, since had there been any anastomosis between that artery and the bronchial plexus, the latter could not have failed to have been injected when vessels of such extreme minuteness as those above described were filled. If the bronchial tube, after it has been thus denuded of its lining membrane, be then subjected for many weeks to maceration in salt and water, and if the flocculent tissue which continues to float upwards be daily, or several times a day, removed by means of soft brushes, (those used for painting on velvet are such as I have employed,) at length the fibro-cartilaginous structure of the bronchial tube will alone remain, and then the minute distribution of the bronchial artery will become apparent, and the fibro-cartilaginous structure of the bronchial tube will be found to be minutely injected from it.

On the other hand, in those specimens in which the pulmonary set of vessels, and not the bronchial arteries, have been injected, it is found that the fibro-cartillaginous structure of the tubes is not in the slightest degree injected, however minutely the plexus surrounding the air-cells, and that lining the bronchial tubes. may have been filled, proving that there is, likewise, no anastomosis in this situation.

Whenever the pulmonary veins are injected with fluid, so as to spread at all minutely, it is impossible to avoid injecting the membrane lining the bronchial tube—the connexion is intimate and direct. The distribution, also, of the vessels lining the bronchial tube, which are filled by this means, is totally distinct from, and unlike to, anything that prevails amongst the capillary vessels in connexion with the bronchial artery in any part of its ramifications. Nothing can be easier than to inject the plexus lining the bronchial membrane from the pulmonary veins, but, as I have said before, it is wholly impossible to do so from the bronchial arteries.

Furthermore, it is not difficult to inject the bronchial mucous membrane from the pulmonary artery; but in order to do this effectually, it is necessary to tie the pulmonary veins, because it requires a greater force to compel the fluid to flow through the plexus of the bronchial tube than it does to make it pass through those capillaries which surround the air-cells; therefore, unless the precaution be taken to tie the opening of the pulmonary veins, the injection will make its escape in this direction, and the bronchial membrane will be only partially injected.

To sum up these particulars:—The air-cells and the mucous membrane lining the bronchial tubes are supplied from the pulmonary system of bloodvessels, and the basement fibro-cartilaginous structure of the bronchial tubes and the cellular tissue throughout the lungs are supplied by the bronchial arteries derived from the aorta; and these latter have no sort of anastomosis nor connection with the former, but are carefully secluded from them by much flocculent tissue, which requires a considerable amount of dissection in order to be got rid of, so that the ultimate distribution of the branches of the bronchial artery may be clearly demonstrated, when these latter have been injected instead of the pulmonary set.—*Lancet, Feb.* 18, 1860, *p.* 179.

DISEASES OF THE ORGANS OF DIGESTION.

40.—ON THE DIAGNOSIS AND TREATMENT OF ULCER OF THE STOMACH.

By Willoughby F. Wade, B.A., M.B., Physician to the General Dispensary, Birmingham.

[Dr. Brinton in his recent work on "Diseases of the Stomach," states that ulcer "occurs very frequently, that it may generally be detected in the living subject; . . . . . that it is usually curable." With this opinion Dr. Wade agrees, observing that symptoms of a peculiar or special character and grouping are observed in these cases, and that these symptoms differ materially from those observed in ordinary dyspepsia.]

The symptoms which enable us to discover the existence of this lesion are: pain after food, and the length of time after eating at which this comes on. This varies in different cases from a few minutes to an hour or two, but it is pretty constant in any given case; the pain is severe, and burning or lancinating, felt in the epigastrium and in the corresponding region of the back, sometimes radiating into the shoulders or down the legs; it is relieved often by one position, aggravated by the opposite one, thus giving us a clue not only to the existence of an ulcer, but to its site. It usually is not much alleviated till vomiting occurs, and from this the relief is speedy, sometimes instantaneous. The ejecta consists of food, more or less digested, with or without a quantity of thin watery or viscid mucus, and at times blood; the appetite is often unimpaired, the tongue natural or pallid, and thirst often absent; tenderness of the epigastrium is seldom wanting, though its extent is generally limited, so that it may escape notice. Tenderness is not unfrequent at that part of the back, in which pain, when dorsal, exists. There is often an absence of heartburn, pyrosis, sour eructations, flatulence. The regularity of the bowels depends chiefly upon the amount and kind of food taken into and retained by the stomach. This remark applies also to the degree of emaciation present, which is also dependent in a great measure upon the extent of the ulceration. Another very marked peculiarity of this disorder, is the worn and suffering countenance presented by patients who have suffered from it for any length of time, quite different from the nervous or fidgetty look of simple chronic dyspepsia. If the stomach have become adherent to any other viscus or to the parietes, or if, as may happen, there be much inflammatory infiltration of the gastric tissues, we shall possibly discover by manipulation some tumour or percussion-dulness or hardness in the abdomen. When we further reflect that these symptoms continue for months or even years unchanged, both in themselves and in their relations to each other, we have certainly a group of phenomena altogether peculiar, and, as I think, characteristic. Of these symptoms it is customary to describe the hæmatemesis as the most convincing sign of a breach of continuity in the mucous membrane; but when we remember upon how many causes it may depend, it can only be considered valuable as *one of a group*, without the other members, of which it loses much of its significance; and it is upon the *presence of all or the majority of these symptoms* in a given case, rather than upon the prominence or severity of any one of them, that I should be disposed to diagnose the existence of a gastric ulcer. At all events I am sure that we shall benefit our patient by looking upon and treating such a case as one of ulcer of the stomach. As the treatment is a somewhat irksome one, it is beneficial to tell the patient what we think of the nature of the disease, the name of which will probably alarm, and thus tend to ensure obedience to our directions.

The treatment which is more beneficial than all others is the adoption of a purely milk diet; but as a large quantity of milk would produce as much exercise and irritation of the stomach as almost any other aliment, and since the irritability of this organ varies much in individual cases, we cannot specify any fixed quantity. The patient should be directed to take as much at each time as he can do without bringing on the pain. This quantity—at first perhaps not more than a teaspoonful—may be repeated as often as compatible with the ease of the stomach; this is a simple rule by which to decide the amount of nutriment: at first, patients cry out that they shall be starved, and so forth, but they soon find that they gain strength as well as comfort, and indeed it may be well to point out to them that they will gain more strength from the complete digestion of a teacupful of milk, than from eating a mutton-chop which the stomach will reject again in perhaps half an hour. It is sometimes difficult to induce patients to try this absolute diet, but once tried the glorious ease which it produces soon commends it to them.

Patients will have some medicine; otherwise I believe that the plan here recommended is of itself not unfrequently adequate to cure. We cannot well advise a person, whom we limit to a teaspoonful of milk at a time, to take an ounce of mixture thrice a day; I therefore use pills, small in size. The irritability of the stomach in these cases suggests the use of nitrate of silver and opium, of each of which I give about a third of a grain three times a day, with one-eighth of a grain of extract of belladonna, and, if the bowels require it, a little castor oil, or henbane and colocynth. This, with a very careful return to ordinary diet, and, during convalescence, some citrate of iron, will, for the most part readily, cure the cases which I have now described, the symptoms of which indicate, as I believe, the existence of ulceration in the stomach.—*British Med, Journal, Oct.* 22, 1859, *p.* 848.

41.—ON THE TREATMENT OF DYSENTERY.

By W. S. Oliver, M.D., Assistant Surgeon 1st Battalion 60th Rifles, Calcutta.

[The following remarks apply to the treatment of dysentery of a peculiarly chronic character, differing much from that usually met with in Europe.]

I have tried, with varied success, every remedy that experience has suggested, but from no other medicine have I obtained the same satisfactory results as from the following three, no matter what previous treatment had been adopted. Those are pure subnitrate of bismuth, creosote, and balsam copaiba.

The cases to which I have found bismuth peculiarly applicable, are those in which the tenesmus and tormina have ceased; but there still remain colicky pains over the entire abdomen, principally referred to the hypogastrium; and, where the stools are of a dark, pitchy colour and consistence, accompanied with impaired appetite and marked abdominal pulsation, and, I may here remark, that this abdominal pulse in dysentery, as in all other abdominal irritations, has been of the greatest assistance to me as a prognostic guide, from the earliest onset of the disease.

I have been in the habit of administering creosote where there is much irritability of stomach accompanied with an inflated abdomen, tenderness on pressure in both iliac regions, and the stools consist of a frothy, coffee-coloured fluid, mixed with shreds of bloody lymph, and a few small masses of scybala; paroxysms of intense griping are generally present in those cases.

Balsam copaiba has proved a valuable remedy in those tedious cases of chronic dysentery occurring in strumous patients, who seem to have a wonderful tolerance of the medicine. It has also been of marked service in those cases of chronic ulceration of the intestine, accompanied with a stool resembling very much vermicelli soup mixed with large pieces of lymphy excrescences, very similar to flat fungus growths which have become detached from the surface of the ulcers.

The following is the mode in which I have been in the habit of administering these medicines :—

Muriate of morphia, gr. $\frac{1}{4}$ to $\frac{1}{3}$: subnitrate of bismuth, grs. viij. to x.; magnesiæ carb., pulv. aromatici, āā, grs. vj. three times a day. The copaiba is given in 15-minim doses, with henbane and almond mixture every four hours; and the creosote I direct to be taken in two or three-drop doses thrice or four times daily, in effervescence with tartaric acid and soda, the latter being in excess.

Should nausea be caused, as is sometimes the case, when the balsam is first administered, I generally order half a grain of opium to be taken half an hour previous.

The use of the above medicines ought to be continued for at least a week or ten days, should their good effects not be apparent before that time, which I think will seldom be the case. The usual dietetic and local remedies, with mild laxatives, must of course not be dispensed with in those cases, and full opiates at night form a useful auxiliary.

Creosote enemas, too, in combination with opiates and mineral astringents, (as first instituted, I think, by Surgeon Innes, late of this Regiment, and now Deputy-Inspector-General,) will be found one of the most decided means of allaying tenesmus in all cases of dysentery, even in its earliest stages, but more especially when it assumes a chronic nature.—*Med. Times and Gazette, Feb.* 11, 1860, *p.* 151.

42.—*Diagnosis of Cancer of the Liver.*—[In a discussion upon the subject of cancer of the liver, at one of the meetings of the Medical Society of London,]

Dr. Harding mentioned that he now believed one characteristic symptom to be always present in cases of, and to be pathognomonic of, cancer of the liver—that is, a small subcutaneous tumour of the breast; but in the male subject he had found it absent there, though a similar small mass had been found in the subcutaneous tissue of the loins. He had met with it in the breasts of females so affected in five cases in succession. He believed it to be a small mass of cancer; it is often painless, is not, therefore, complained of, and requires to be sought for; it may be the size of an almond or small marble, but at first not larger than a pea; it is not situated in the gland-substance of the breast, but just beneath the skin, and most usually a little to the outer side of the nipple. If he (Dr. Harding) found this small mass, and at the same time discovered any symptoms whatever of hepatic or stomach derangement, he should diagnose positively the presence of cancer in the liver, even if no other sign of this latter condition existed. He had met with one case of cancer of the liver in a child, aged four years.—*Lancet, March* 3, 1860, *p.* 220.

43.—ON THE PATHOLOGY AND TREATMENT OF GALL-STONES.

By J. L. W. THUDICHUM, M.D.

(Read before the Medical Society of London.)

[The author commences his paper with the proposition that the formation of gall-stones must be the result of some pathological cause, either co-existing with, or preceding by some period the actual deposit of these bodies. The nature of this primary disease, or diseases, is unknown, at least with any degree of accuracy or certainty. Then follows a case of heart-disease, dying with anasarca and intense dyspnœa. The age was sixty. The gall-bladder was large and contained numerous gall-stones, but the duct did not seem obstructed.]

On examining the bile in the gall-bladder, I found it to consist of a homogeneous fluid, containing little colouring matter in solution, but a large amount of brownish-yellow colouring matter, together with many crystals of cholesterine were suspended in it. The bile and colouring matters dissolved in alcohol, yielding a yellow solution, which was separated from mucus and epithelium by filtration. The alcohol was distilled off. and the residue tested with sugar and sulphuric acid. But no colour-test could be obtained with the greatest care, and although the test was repeated several times: *there was not a trace of biliary acid in this fluid.* But it contained some margaric and stearic acids, which, together with the cholesterine, crystallized from the concentrated extract.

The gall-stones were sixteen in number. On dividing the largest one into two halves, I found a large nucleus of brown pulpy matter, which could easily be removed with the point of a knife, or washed away with a brisk stream of water from a wash-bottle, or so-called blow-bottle. When collected in a white china dish, the matter appeared to be composed of thread-like fibres, of different diameters, some a quarter of an inch long; some shorter pieces were one-sixteenth of an inch in thickness. All were cylindrical, as if moulded in tubes; many had branches, others were divided dichotomically. The thinnest portions had a diffuse broom-like end, as if the matter had not had time or quality to solidify in the tubular form, or as if it had solidified in a bag-like enlargement of the cylinder, in which the rest of the cast was moulded. The matter composing these productions was granular, without a trace of crystallization of any kind; was purely yellow in the thinnest branches, but became darker brown, the thicker the forms grew. All further description becomes at once unnecessary, by the faithful representations of the more striking portions which I have caused to be executed by Mr. Lens Aldous. Some forty or fifty medical gentlemen, to whom I had an opportunity of submitting both the specimens and their drawings, have borne witness to the accuracy of the representation. All of them agreed with me that these peculiar formations can be nothing else but *casts of the biliary ducts.*

I had often searched human livers for some analogue to the casts of the uriniferous tubules, but in vain; and I should certainly not have dreamed of finding them in the centre of gall-stones. I have since examined many, but only old and dry gall-stones; most of them contain a nucleus of colouring matter, which has very great analogy to the matter forming the casts, but their shape I have not found repeated. It is indeed a mere chance whether such casts shall be preserved, or whether they, on being discharged from their place of formation, shall pass into the intestinal canal. And even where they arrive in the gall-bladder, and become entangled and covered with cholesterine, evidently a secondary occurrence, it is again a mere chance whether they shall not be so surrounded and interlaced by cholesterine crystals, that, on breaking the stone, they must necessarily come to pieces. They are so fragile, that the mere weight of a thin glass cover, as used for microscopical preparations, was sufficient to crush the thinner ones. When shaken in the watch-glasses, in which I kept them, moistened with glycerine, the mere friction of one against the other would damage and disintegrate the most characteristic feature. About half the number of gall-stones, and among them some large-sized ones, although containing a good brown nucleus, yet did not admit of the separation of characteristic casts, although among the *dèbris* I could distinguish fragments of casts with ease and certainty.

The material of the casts was not chemically homogeneous. Alcohol extracted a yellow portion; another portion, probably bilifulvine, remained. Some had a peculiarly ragged or variably projecting outline, which made me examine for cylindrical epithelial cells; but, however great the probability that such cells might adhere to the circumference, or enter into the body of the casts, being epithelium proper to the biliary ducts, certain it is that no such formations could be identified.

Numerous are the reflections suggested by this observation. The pathological process which effected the effusion of a congealing colouring matter into the biliary ducts was probably an acute one; if it involved only a portion of the liver, it probably presented itself at the time under the form of a true bilious attack, that is to say, a derangement of digestion, fever, headache, and a yellowish colouration of the skin, and albuginea of the eyes. If the process involved the greater part or the whole of the biliary ducts, it must have shown itself under the form of acute jaundice. It is not impossible, or rather probable, that those forms of idiopathic jaundice which hitherto have been ascribed to spasmodic closure of the duct, or to failing secretory activity, a sort of paralysis of the liver may hereafter find an explanation by the discovery of a real and material obstruction of the passages of the bile by formations similar to those which I have described. Frerichs saw a case of jaundice coming to a favourable crisis by the discharge of brown flakes and threads, which were very conspicuous upon the light coloured fæces, and ushered in the reappearance of the ordinary brown colour of the evacuations.

The branched calculi of Plater, and the branched gall-stones which Fauconneau-Dufresne in one instance discovered in the finer ramifications of the biliary ducts, have perhaps some analogy to the casts of the bile-ducts brought under your notice.

In continuing for a moment the consideration of the etiology of gall-stones, I beg to offer some fragmentary speculations on the possible or probable circumstances which may produce gall-stones. It remains yet to be ascertained whether a foreign body in the gall-bladder can simply predispose the cholesterine to crystallise upon it, or whether a change in the composition of the bile must precede that separation, as alkaline urine must precede the phosphatic incrustation of foreign bodies in the urinary bladder. There are cholesteric calculi containing pins, flukes, or ascarides as nuclei. I shall have an opportunity of bringing some experiments on this question before you on a subsequent occasion, as it seems to admit of a satisfactory solution. At present we must allow, that the nucleus being given, whether it be a foreign body, or casts of the biliary ducts, or amorphous precipitated colouring matter, or blood, or cast-off epithelium, or even the problematical mucus—the subsequent crystallization of cholesterine may be either a consequence of the effects of the nucleus upon the bile and gall-bladder, or a continuation of the cause which produced the nucleus. The casts may have acted as foreign bodies simply, or the liver-cells, which effused the abnormal congealing matter, may have thrown out an excess of cholesterine, or bile incapable of holding the ordinary quantity of cholesterine in solution.

As there are gall-stones which possess no tangible nucleus, but consist throughout of pure pearl-white cholesterine, the circumstances under which cholesterine may crystallize from bile, are particularly deserving of investigation. Excess of cholesterine merely, which Chevreul thought to have found, though a very likely thing to occur, is in itself not sufficient to explain its precipitation; a change or destruction of its solvent, taurocholic acid, on its passage through the ducts and bladder, seems essential. This change may be produced by specific disease of the liver-tissue, by retention of bile and subsequent endosmotic changes, or by decomposition of bile from any cause. Bile, whether stagnating in the human body, or in a glass bottle, on decomposition setting in, gives up its cholesterine in a crystallized form.

This decomposition of the solvent of cholesterine, once set up in the gall-bladder, continues, like ammoniacal urine in the urinary bladder, to contaminate all fresh portions of bile poured out into the gall-bladder, and to precipitate their cholesterine, and often their colouring matter upon the centres of attraction. In this manner are formed the layers of gall-stones, which in their concentricity exhibit the periodical growth of the concretions. Mixed with the cholesterine layers are patches or layers of colouring matter, namely, bilifulvine and biliverdine, in various proportions and shades. Bistearate of lime in starlike crystallized small masses, and a little phosphate of lime mixed with shrivelled cylindrical epithelial cells, make up the list of constituents which we observe in the greatest number of ordinary gall-stones. Concretions, consisting of pigment, or inspissated bile, or of phosphate and carbonate of lime, have a different pathological, and by their rarity, less practical significance.

In the *symptomatic treatment of the passage of gall-stones* we have to rely mainly upon opium, which is sometimes better borne in the form of pills than that of tincture. We must carefully guard the patient from an overdose of this drug, which he is even more apt to take on his own account, from the torturing pain he is suffering, than the practitioner is likely to prescribe it. Nevertheless, severe narcotism sometimes follows large doses of opium—when the pain which caused them to be given suddenly ceases, from local causes. Such a case happened in Ireland some years ago, and was the subject of a painful legal inquiry. Happily, the life of the patient, who was a member of Parliament and a large estate proprietor, ended fatally only a few days after the severe narcotic symptoms had been successfully combated, so that the death and the narcotism stood in no immediate connection with each other. Whatever be our opinion, this case suggests caution.

Persons afflicted with dormant gall-stones should regulate their living in the most careful way possible, avoid all and every description of spirituous liquors, and let fruit and cereals, and light vegetables, enter largely upon their dietary.—*British Med. Journal, Nov.* 19, 1859, *p.* 936.

DISEASES OF THE URINARY ORGANS.

44.—OBSERVATIONS ON SOME OF THE DAILY CHANGES OF THE URINE.

By William Roberts, M.D., Physician to the Manchester Royal Infirmary.

[The opinion held previous to 1849 was, that the reaction of healthy urine was necessarily acid. At that time Dr. Bence Jones propounded the hypothesis that the stomach and kidneys were, as to the reaction of their respective fluids, in a state of antagonistic correspondence, i.e., when the gastric juice was most actively secreted, the urine was least acid, or slightly alkaline, and *vice versa.* This hypothesis, however, though apparently well supported by experiments, has not been found correct by subsequent observers, as Dr. Beneke, Vogel, and Dr. Sellers, of Edinburgh.]

It seemed, therefore, not undesirable, in the face of this contradictory testimony, to submit the question to a new investigation, and to inquire if means could not be found to reconcile the conflicting facts, and ascertain with some precision what effect food had on the reaction of the urine.

To this end the urine was examined at short intervals,—after a meal, usually every hour; and at periods more remote, every two hours. The hourly flow being thus ascertained, the absolute quantity of free acid discharged per hour was estimated by a test solution. Dr. B. Jones, in his experiments, was content to gauge the amount of acid in 1000 grains of the urine, without reference to quantity per hour; and it was therefore objected to them, that the varying concentration of the secretion, especially the great dilution which follows free potations, vitiated the conclusions deduced. The objection, however specious, lies with very little justice against these researches; for Dr. Jones was always careful to specify the density of every urine which he examined, and in this way to guard against any very great fallacy from unequal concentration. The method of hourly estimates has, however, unquestionably the merit of superior exactness; hence it was adopted in the following observations.

The mode of proceeding was to heat five hundred grains of the urine, and to add gradually a dilute solution of caustic soda to saturation. If the urine became alkaline, dilute sulphuric acid was used in the same way to ascertain the degree of alkalescence. Both test solutions were made of equal saturating powers, each 100 grain measures being equivalent to one grain of dried carbonate of soda. In this way the acidity or alkalinity per 1000 grains was ascertained; and from this, and the quantity of urine secreted during the hour, the hourly amount of free acid or free alkali discharged was computed.

The results of these experiments confirmed in the fullest manner the conclusions of Dr. Bence Jones, that a meal, be it of animal, vegetable, or mixed food, has a powerful and constant effect in lowering the acidity of the urine, frequently even rendering it alkaline.

The mean results obtained are unequivocal in their affirmation of the power of mixed food to depress the acid reaction of the urine; and the one set is remarkably in agreement with the other. The urine became alkaline for about two hours after breakfast, and for about three hours after dinner. The several days differed a good deal from each other as to the intensity of the effect. The urine did not always change from acid to alkaline after a meal, but it invariably showed a greatly lowered acidity. Some of the causes which operate to produce these inequalities will be presently adverted to.

On some additional days four meals a day were taken, according to the usual mode of life of the subject of experiment, and malt liquor was taken with dinner and supper. This did not alter essentially the results obtained; but a modification was caused by the quick succession of the meals. for it was found that the *remote* effect of one meal interfered with the *immediate* effect of a succeeding one.

Two sets of experiments were also made with a diet of purely *vegetable food*. In the first of these, the days alternated with days on which animal food was used. The experiment, too, only embraced a portion of the twenty-fours, namely, from 7 a.m. to 11 p.m. The second set embraced five complete days, of which three were consecutive; the other two succeeded to days of mixed diet, on which dinner had been taken at 2 p.m., after which no solid food was taken until breakfast next morning. It may be observed that a very considerable difference exists between the results of the two sets;—that on the days composing the first, the urine never became alkaline at all. and even the depression of the acidity was comparatively slight; whereas on the days of the second set, vegetable food showed a very powerful influence over the urinary reaction. The articles of diet used were bread. sugar, coffee or tea, without cream, for breakfast; and bread, potatoes, rice, and a vegetable soup for dinner. Occasionally carrots and celery were added.

In the last place, purely *animal food* was tried; and it was found most efficient in depressing the acid reaction of the urine.

I could not perceive any distinct difference in the effect of the diverse forms of animal diet; fish, flesh, and fowl and game appeared to act with equal power.

The conclusion, then, to which these experiments lead, is that ordinary food, whether it was purely animal, purely vegetable, or, as was more usual, an admixture of the two, invariably caused a diminution of the free acid separated by the kidneys. This conclusion is the more remarkable, as it scarcely squares with what is known of the reaction of the urine in carnivorous and herbivorous animals. In the former, the urine is described as always acid, and in the latter, as invariably alkaline; and the difference is attributed to diversity of food in the two classes. The experiments of Dr. Cl. Bernard seem conclusive on this point. He found that when rabbits (whose urine is normally alkaline) were fed for some time on an exclusively animal diet, they passed an acid urine, and that its alkalescence was not restored until a vegetable diet was substituted. Dogs also, when restricted to a vegetable fare, secreted an alkaline urine, turbid from deposition of phosphates; but when restored to animal flesh, their urine resumed its natural clearness and acid reaction. How to reconcile these contradictions, I am unable to say; but it may be pointed out as worthy of notice, that of the three *consecutive* days of exclusively animal and exclusively vegetable diet, the greatest effect in the former was on the first day, and it fell progressively on the second and third days; whereas the exact reverse took place on the days of vegetable food.

Another point was brought out with great distinctness in these observations, namely, the falling off which takes place in the hourly amount of free acid after prolonged fasting, and the still greater falling off in the hours of sleep. But this second decline has a totally different origin and significance from the depression that follows a meal. The latter is a true *alkaline tide*, and if it but flows strongly enough, the urine altogether loses its acid reaction and becomes alkaline; but in the former case the intensification of the phenomenon has no tendency to change the reaction of the urine, because it depends simply on the diminished secretion of solid urine, which, as we have already seen, invariably accompanies the state of fasting and sleep. That this is the true interpretation of the fact, may be seen by comparing the degree of acidity per 1000 grains of liquid urine; or, still more exactly, by comparing the amount of free acid corresponding to 100 grains of solid urine with the amount separated per hour. It is seen that the *degree of acidity* of the urine steadily rises, after recovery from the depression of the alkaline tide, until the next meal.

I was at first led into error by studying the oscillations of the free acid too exclusively from the hourly point of view. It appeared as if an *acid tide* followed immediately on the ebb of the *alkaline tide*; for the hourly secretion of acid was uniformly found at its highest shortly after the urine had recovered its acidity; but this was entirely due to the fact, that the increased activity of the kidneys, called forth by the meal, persisted for two or three hours after the normal acidity of the urine had been restored. There seems, however, a limit to the increase of the acidity in prolonged fasting; for Dr. B. Jones found, that continuing to fast for twelve hours beyond the usual time of taking food did not intensify the acidity of the urine.

There is evidently the closest relation between the reaction of the urine and that of the blood. By adding to the alkalescence of the blood through artificial means, as by exhibiting caustic or carbonated alkalies, we are able to depress in a corresponding proportion the acidity of the urine. On the other hand, also, by exhibiting acid (although this seems less readily accomplished), we can similarly heighten the reaction. We may, therefore, look on the urine as a measure of the state of the blood. When the urine is highly acid, the alkalescence of the blood is below par; when on the contrary, the urine is alkaline, it is an indication that the alkalescence of the blood is above par. It follows, therefore, that to the kidneys is delegated the important duty of regulating the reaction of the blood. When the blood tends to become too feebly alkaline, the kidneys separate acid largely; when, on the contrary, its alkalinity mounts beyond the physiological requirement, the same organs secrete a feebly acid, or even an alkaline urine.

The alkaline tide—and in this term I would include the whole period of depressed acidity, and not merely the time during which the urine is actually alkaline—was found to set in earlier after breakfast than after dinner, and its duration was more brief. After breakfast the acidity showed nearly always a sensible declension in forty minutes.

The urine, however, never became alkaline, nor even neutral, so soon During the second hour after breakfast (from nine to ten) the alkaline tide usually culminated; but in about a third of the cases the point of least acidity was not reached until eleven o'clock. Then the tide. turned; and from eleven to twelve the urine was found fast recovering its reaction, and about one the normal level was usually attained. Thus, for about four hours the morning meal exercised a depressing effect on the acidity of the urine; but it was not actually alkaline usually for more than an hour, sometimes for two hours, and once for three hours.

The effect of dinner was not perceptible until the second hour after the meal. During the next three hours (third, fourth, and fifth hours) the alkaline tide ran in its greatest strength. On the third and fourth hours the urine was always, (with two exceptions) found alkaline when the meal had been of mixed food or animal diet. At the end of the sixth hour the tide had generally turned, and the acid reaction been restored. Three hours was the usual duration of the alkalescent state after dinner; sometimes two hours, more rarely four hours, and on one occasion five hours. The amount of free alkali discharged after dinner was, generally, not far from double the quantity observed after breakfast; so that, in duration and intensity, the effect of dinner proved about twice as great as that of breakfast. The difference arose, probably, simply from the fact that breakfast was a much lighter meal than dinner, and its impression on the system consequently smaller.

The alkaline urine that was passed after food owed its reaction to a fixed alkali, and not to ammonia. It did not effervesce with acids. It was rich in earthy and alkaline phosphates; and on these latter, in a basic state, depended apparently its alkaline reaction. As might have been anticipated, the loss of acidity entailed the precipitation of the earthy phosphates; and the urine, when passed, was frequently turbid. But this was not always so. Not unfrequently, especially after breakfast, the urine, although alkaline, retained its transparency. Generally, such a urine was of feeble alkalinity and dilute; but now and then it was observed to be tolerably concentrated, highly alkaline, and still clear. All transparent alkaline urines were rendered immediately turbid by caustic ammonia, and by mere heating; so that the transparency did not depend on the total absence of earthy phosphates. It was also found, that in the turbid urines, after subsidence of their deposits, caustic ammonia caused an additional precipitation.

The degree of turbidity varied from a barely perceptible cloudiness to a thick muddy opacity. The deposit subsided quickly, and left a clear, yellowish-amber supernatant liquor, often with a greenish tinge.

The odour of this alkaline urine resembled that of the fresh urine of the horse. It had lost the characteristic urinous odour, and exhaled a strong sweetish aroma, so peculiar as to indicate with certainty the change of reaction without the aid of test-paper.

The urine of the alkaline tide was rich in uric acid (as calculated per 1000); rather more so even than the night urine, which nearly always let fall a copious urate deposit on cooling. The hourly quantity was almost three times greater than during the succeeding period when the acidity was restored (from nine to eleven), and more than three times greater than during sleep. The differences are not so great when the uric acid is reckoned as a percentage on the solid residue; although here also the alkaline urine shows notably the highest proportion.

The remarkable poverty of the urine in uric acid from nine to eleven, was probably a direct result of the excessive separation during the preceding hours of the alkaline tide; the blood having been thereby left poor in uric acid, and therefore unable to supply the kidneys with the usual quantity.

Although, as we have seen, the immediate effect of a meal was to depress the acidity of the urine, the *more remote consequence* was to uphold, and even to increase the acidity. This was seen most distinctly when comparison was made of the acidity of the urine on mornings succeeding supperless nights, and mornings following a hearty supper. In the former case, the mean hourly rate of acid discharged between 7 and 8 a.m. was only 0·51; in the latter, it was 0·88, or nearly double. And not only was the hourly discharge thus increased, but even the degree of acidity per 1000 showed a distinct exaltation,—the mean numbers being, for the mornings after supperless nights, 1·83, and for mornings after a supper, 2·15. This latter reckoning, however, did not always exhibit results in accordance with this general mean, and exceptions occurred; but for the *hourly* reckoning all the separate results were consistent, sometimes in a greater, sometimes in a less degree, with the general mean.

The remote effect of animal diet appeared considerably greater than that of vegetable food. So that a highly animalized diet would seem to tend in the long run to heighten the acidity of the urine; a conclusion quite in harmony with ancient belief. Here, no doubt, lies the explanation of the otherwise puzzling difference between the effect of vegetable food in the first and in the second set of experiments on that diet. In the first set, the urine showed great unsusceptibility, not once losing its acid reaction, nor even reaching the neutral line; whereas, in the second set, it became alkaline, and even strongly alkaline, on several occasions. In the first, a diet rich in animal flesh had been used on the alternate days; and the remote effect of this evidently overpowered the immediate effect, that would otherwise have shown itself more prominently. This circumstance is of importance to bear in mind when the effect of food on the reaction of the urine is to be traced. A heavy supper the night before, especially a meat supper, will greatly diminish the effect of breakfast next morning. For this reason, it is well, in order to obtain distinct results, to fast for eight or ten hours, or more if night intervene, before taking the meal whose effect it is wished to observe.

It was a marked feature in these experiments, that although there was the greatest constancy and certainty in the successive changes of reaction, there were very considerable, indeed, the widest, differences in the *absolute amounts* of free acid or free alkali separated by the kidneys at corresponding hours after a meal on different days. Whence these differences? This question may be conveniently considered in connection with another. Is the action of food, in lowering the acidity of the urine, a universal phenomenon? It is impossible to give a complete answer to this question until observations on separate individuals have been greatly multiplied; but the facts already known warrant the statement, that at least some healthy persons, whose urine otherwise appears perfectly normal, suffer constantly a diminished acidity of urine after meals, and often emit a urine which is alkaline and turbid, for four and six hours daily, without the least evidence of impaired function.

I am disposed to believe that the depression of the acidity of the urine after a meal is of universal occurrence; but at the same time observation has convinced me that there are very great differences in the degree or intensity of the phenomenon in different individuals. The source of these differences may be looked for in certain fixed peculiarities of constitution; and among these, the following may be indicated as probably operative to that effect:—

1. *Differences in the Amount of the Insensible Perspiration.*—As the sweat is always acid, individuals with a moist active skin would separate through this channel a large quantity of acid, and consequently leave less for the kidneys to remove. In such persons, it might be expected that the alkaline tide would present more prominence than where the cutaneous surface was habitually dry and hard. Whether the sweat, under any circumstances, loses its acid character or not, I am unable to say; but I have invariably found it sour, even when the urine has been kept continuously alkaline by bicarbonate of potash for several weeks successively.

2. *The exaltation which takes place in the respiratory changes after a meal.*—The effect of an increased absorption of oxygen would be to increase the formation of acid. Those persons, therefore, in whom this exaltation takes place promptly after a meal, and is unusually great, would probably show but a feeble alkaline tide, because the acid-producing power of this excess of oxygen would counteract the opposing tendency of the meal.

3. *A power of quick digestion and absorption* would probably enhance the effect of a meal, inasmuch as the more suddenly a meal is thrown into the channels of the circulation, the greater the disturbance it would temporarily cause.

4. I am also of opinion that an overworked or "below par" state of system is promotive of a diminished acidity of the urine; and that a high state of health and vigour tends to a heightened acidity.

In addition to these modifying conditions, which apply specially to different individuals, but also in a less degree to the same individual at different times, there are other circumstances which have a stronger bearing in the latter case. Of these, the most powerfully operative is the remote effect of a previous meal, which has already been fully considered. There is also the condition of exercise or rest. It is thought that, in a state of muscular activity, there is an increased production of the acid muscular juice; and this transuding into the blood, and passing through the kidneys, would, as Vogel surmises, probably intensify the acid reaction of the urine. And lastly, the richness or poverty of a meal in basic phosphates may, almost with certainty, be named as having great influence on the strength of the alkaline tide.

Some of these considerations have scarcely an intelligible force until the next inquiry is answered:—*Why should a meal diminish the acidity of the urine?* And to the solving of this question our attention must now be turned.

Dr. Bence Jones' view has already been explained. The acid generated in the blood has become diverted to the stomach to digest the food; hence there is less—or none at all—to pass away by the urine. There are some difficulties, however, in the way of adopting this as the *sole* explanation of the diminished acidity of the urine after food; and another theory is here offered, based on totally different grounds, which appears to square better with the facts ascertained.

It seems from the observations of Dr. Beaumont on Alexis St. Martin, that the gastric juice required to digest a meal is poured into the stomach with great celerity; so that in twenty minutes, half an hour, or an hour at most, the whole quantity demanded is secreted, and no further flow takes place. Absorption then commences, and the acid begins to return with the chyle back again into the blood. If this be so, the reaction of the urine ought, on Dr. Jones' hypothesis, to reach the point of greatest depression in an hour or an hour and a half after a meal, instead of from two to five hours after. Moreover, how may it be explained, on this supposition, that the alkaline tide after dinner sets—in an hour or an hour and a half later than after breakfast? Besides all this, the acid of the gastric juice and that of the urine are certainly not identical, nor apparently even similar. These objections, however, although not without considerable force, would not suffice to upset a theory so plausible, were there not distinct reasons, and, as I believe, sufficient ones, to adopt an altogether different explanation: which I will at once proceed to lay before the reader.

We have seen that the alkaline tide coincided, in point of time, with the increase in the secretion of solid urine; may not the alkaline tide, therefore, be considered concomitant with the absorption of the meal into the blood, rather than with its digestion? But how can the passage of a meal into the blood affect the reaction of the urine? *By raising temporarily the alkalinity of the blood through the means of its mineral constituents.* The blood of all animals is alkaline; and Liebig informs us, that this arises from the fact, that "all articles of food which alone, as bread and flesh, or when mixed with vegetables, are capable of sustaining the process of sanguification and nutrition, contain carbonic acid, or phosphoric acid, and alkalies,—the two latter in such proportion, that if we suppose them dissolved, the alkalies invariably predominate." Every meal, therefore, that is dissolved and absorbed into the blood, increases the alkaline reaction of that fluid, and raises it for a time above the natural level. But, as already explained, changes in the reaction of the blood are immediately reflected in the urine. A basic alkaline phosphate or a carbonate artificially introduced into the blood, inevitably lowers the acidity of the urine, and, if in sufficient dose, renders it alkaline. What then is to hinder the carbonates and basic phosphates of our food from acting similarly? A meal, consequently, may be regarded from this point of view as a dose of alkali, which first exalts the alkalinity of the blood, and as a more remote, but equally certain consequence, diminishes the acidity of the urine.

In harmony with this explanation is the fact, already pointed out, that the increase of the solid urine (which is an index of food passing into the blood) takes place simultaneously with the declension of the urinary acid. It is true that the subsidence of the alkaline tide is not synchronous with the completion of absorption; for the former begins to ebb while absorption (to judge by the quantity of solid urine) is still proceeding actively. This want of coincidence appears *primâ facie* to militate against the solution here offered; but it may be explained in two ways. Either it arises from the saline constituents being absorbed with more celerity than the rest of the food; or, more probably, it depends on the increased absorption of oxygen by respiration, already noticed as occurring after a meal, which, after the lapse of five or six hours, by generating acid, counteracts the contrary effect of the food—in other words, from the remote effect of a meal overlapping the immediate effect.

When food devoid of mineral constituents was used, there was no lowering of the acidity of the urine. On two days sugar or honey was taken instead of ordinary food, and the acidity of the urine maintained itself almost unchanged, with a tendency to elevation rather than depression.

It must not be supposed, however, that the urine is never alkaline from fixed alkali, except as a consequence of taking food. Clinically, the urine is observed not very unfrequently in this condition. Dr. B. Jones has related such cases; and I have once and again met with them myself. In persons of a debilitated constitution, especially in the anæmic state which sometimes follows obstinate subacute rheumatism or gout, and in chlorotic females, I have seen the urine alkaline for a day or two, during the time of fasting. Next day, however, the urine would return to its natural state; reverting again, perhaps in the course of a week or ten days, to the alkaline state.

According to Homolle and Duriau, the urine becomes alkaline by immersion of the body in a bath. It is even stated that the addition of nitric acid to the water does not prevent the urine of the bather from losing its acidity.

Finally, let us offer some considerations which may explain why the alkaline urine of food is not oftener met with in actual experience; and why there should exist such important discrepancies between the different observations made in relation to it.

The great source of fallacy is neglecting to isolate the product of the alkaline tide from the acid urine secreted before and after its flow. To do this, it is necessary to empty the bladder at the commencement and at the close of the period during which the urine is alkaline; and from the uncertainty of the time after a meal at which the point of greatest depression is reached, this can only be accomplished by examining the urine at hourly intervals. This is the reason, I apprehend, why the observations of Beneke and Vogel, and probably also those of Dr. Sellers, are at variance with those of Dr. Jones and myself.

In Dr. Beneke's experiments, the bladder was not emptied oftener than about five times a day; and by comparing the times of the meals with the times of micturition, it is evident that it was impossible for him to obtain results other than nugatory. The urines which he examined were mixed urines, and he did not in any wise isolate the secretion at the critical periods.

The same objection appears to lie even more strongly against the observations made under the supervision of Vogel. The urines were collected for three periods—namely, between breakfast and dinner (morning urine); between dinner and evening (afternoon urine); and during the hours of the night. All such urines would be acid mixtures; but it by no means follows that they did not mask an alkaline state, even it may have been, of some hours' duration.

Nothing could more strongly prove the importance of frequent examination of the urine, than the fact that the subject of these experiments was not in the least suspected to void at any time an alkaline urine until the urinary product was, as it were, analysed by hourly examinations; and this although the urine had been under observations for years. And it took me by surprise to find the phenomena of the alkaline tide so strongly and so remarkably pronounced, after such long and effectual concealment.

5. *The Appearance of the Urine.*—Dr. Prout tells us, "that in a perfectly healthy condition of the urine, this secretion not only remains transparent after cooling, but continues transparent until it bekins to suffer those spontaneous changes incidental to all fluids containing organized matters in solution," This is a much too abso-

lute statement even for the urine of ordinary micturition; and is certainly quite erroneous in relation to the urine as it leaves the kidneys.

The highly acid and concentrated secretion which is separated after six or eight hours' fasting, may be *expected* to let fall a deposit of amorphous urate on standing a while. I found the urine secreted between seven and eight in the morning (before breakfast), *nearly invariably*, to become sedimentary on standing a few hours; and usually even on mere cooling. I cannot, therefore, subscribe to the doctrine that the occurrence of a urate deposit *necessarily* indicates some departure from the healthy state. A low temperature, with a concentrated and highly acid state of the urine, are of themselves sufficent to throw down the urates. I have never witnessed a urine turbid from urates *when passed.* Dr. Prout states, that in gouty persons he has observed such urine; at the same time, he signalizes the extreme rarity of such a condition. Dr. Thudichum relates a most singular example. A child suffering from fever had retention of urine: and when the urine was withdrawn by the catheter, it was found loaded with a lumpy deposit of urate of soda.

Like Dr. B. Jones, I found that the occurrence of a urate deposit on cooling was by no means a sign of excessive discharge of uric acid; on the contrary, the urine of the alkaline tide (which never deposited urates) was found to be in every respect richer in uric acid.

An alkaline urine depositing earthy phosphates is always turbid *when passed;* cooling does not increase the sediment; indeed, I have thought that a very slight cloudiness from this cause has disappeared on cooling, and left the urine transparent. At first sight, it might seem that the presence of urine containing suspended flakes of earthy phosphates in the urinary passages, would lead inevitably to the formation of concretions either in the renal tubuli or in the bladder; but it is notorious that the primary formation of a spontaneously-arising urinary concretion never is phosphatic, but nearly always composed of uric acid or oxalate of lime. And I am disposed to believe, rather, that the occurrence of an alkaline tide after meals is an admirable provision of nature for diminishing the risk of calculous formation.

After long fasting, and during the night, the secretion that flows through the kidneys is very acid and very concentrated—in a condition most favourable to the precipitation of crystals of uric acid or oxalate of lime; but if any such fall, they are dissolved and washed away by the alkaline tide after breakfast or dinner. The earthy phosphate deposit has itself no tendency to aggregate in masses, nor to adhere to any foreign body. When, however, ammonia begins to be developed in the urine, crystals are formed, and the deposit shows strong tendency to run into masses or small concretions. This deposit, therefore, as long as it is free from ammonia—and it is free from ammonia, as it appears, in the alkaline tide after meals—can safely pass through the urinary passages without fear of its forming a calculus.

The colour of the urine was found to vary chiefly in degree; and, for the most part, simply from greater or less concentration. The night urine, however, was always noted as having a reddish tinge, and the urine of the alkaline tide had often an olive cast.

Healthy urines seem divisible into two distinct classes,—1. *Urines of Fasting (urinæ sanguinis),* and 2. *Urines of Food (urinæ cibi).* The former are highly acid, inclined to deposit urates on cooling, but always clear on passing; apt, also, to be high coloured, especially during sleep. Estimated according to the amount of solid constituents discharged per hour, they are scanty. The urines of food fall into two well-marked varieties—those with a *diminished acidity*, and those with a *restored acidity.* Both varieties are of weak acidity, and abundant in quantity. They show no tendency to deposit urates on cooling, although they are rich in uric acid. They are rich in earthy and alkaline phosphates also; and the alkaline variety is usually turbid when passed.

Either class may be watery or concentrated. according to the relation between potation and the requirements of the system as respects water. The *urinæ potus*, therefore, do not merit to be regarded as a distinct class.

Very frequently the urine filtering through the kidneys is of a transitional character, as urine of one class or variety passes into another. The urine of micturition almost always belongs exclusively to no division, but is a mixture of several kinds; hence it scarcely ever presents, in well-marked degree, the special features of the urine of fasting or of the urine of food. Perhaps, if this classification of urines were kept more in view, the method of estimating the solid constituents from the density would yield more accurate results.—*Edinburgh Med. Journal, April* 1860, *p.* 906.

45.—ON URIC ACID AND URATES.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

Uric Acid and Urates are present in certain proportion in healthy urine, but in disease a large increase is very frequently observed. The substances occur as urinary deposits, either from existing in too large a proportion to be dissolved in the urine when cold, or, as is probably the case in the majority of instances, from the development of an acid in the urine, which causes them to be precipitated from their solutions. In many acute febrile diseases, the proportion of uric acid is increased, and the period of resolution of inflammation is marked by diminished frequency of the pulse and respiration, by a fall in the temperature, by free perspiration, and by a very abundant deposit of urates. In health, from five to eight grains of uric acid are excreted in twenty-four hours; but, in some acute diseases, the proportion may amount to twenty grains. In a case of fever, Dr. Parkes found that 17·28 grains of uric acid were excreted in twenty-four hours. Urate of soda is very readily caused to deposit crystals of uric acid. If the amorphous deposit be merely dissolved by warming the urine, the urate often becomes decomposed; and, as the solution cools, crystals of uric acid are deposited. In some cases, the quantity of uric acid held in solution is so great that, upon the addition of a drop of nitric acid to the urine, an abundant amorphous precipitate, exactly resembling albumen is formed. Such a precipitate has many times been mistaken for albumen, and, even if examined under the microscope immediately after it is formed, its nature cannot be made out; but if it be allowed to stand for some time, the amorphous particles gradually increase in size, and assume the well-known crystalline forms of uric acid. The instances in which I have met with urine exhibiting these characters have almost all been cases of liver-disease. Although the reaction is acid, no precipitate takes place upon the application of heat, which at once distinguishes urine of this character from albuminous urine.

The presence of an abnormal quantity of uric acid in the urine shows that more of this substance or its salts is formed in the blood than in health. It would appear that, in consequence of certain conditions, a large proportion of the uric acid resulting from the disintegration of albuminous substances is not further oxidised and converted into urea, but combines with ammonia, or lime, forming urates of these bases. In gout, the presence of uric acid has been detected in the blood by Dr. Garrod. *During* the attack there is less in the urine than in health; but *after* it is over, a large quantity of uric acid and urates are often carried off from the system in the urine.

In cases characterised by a tendency to the formation of much uric acid, the principal objects to be attained by treatment are, to favour the further oxidation of the uric acid formed, and to promote its solution and elimination from the blood as rapidly as possible. Good air and moderate exercise, with attention to the action of the skin, will fulfil the first object; and the solution and elimination of the urates will be encouraged by giving alkalies in solution in a considerable quantity of water.

The satisfactory change which in chronic gouty and rheumatic cases frequently ensues from following some of the much vaunted "systems," or going through a course of bathing in Germany or elsewhere, obviously arises from the increased action of the skin, and the improvement of the health generally, effected by the exercise, good air, simple diet, and temperance, wisely enforced in the establishments. If patients could be induced to retire to a pleasant part of the country, where they could take moderate exercise and be free from mental anxiety, meet with agreeable society, live regularly, take small doses alkalies, and soak themselves for an hour or two a day in warm water in which some carbonate of soda had been dissolved, they would receive as great benefit as by travelling hundreds of miles away, and at much less trouble and expense. I am convinced that there are many patients who would prefer to carry out such a simple plan, rather than submit themselves to all the useless routine and absurd formalities involved in many of the professed universal systems, such as homœopathy, hydropathy, &c., which cannot but be extremely offensive to their common sense, while they are claimed as converts and supporters of doctrines which they do not really believe in. There are many who, for the sake of the advantage they derive from the regular system of living, air, exercise, &c., express no disbelief in doctrines and propositions which they probably feel to be absurd, and which a little reflection must prove to be false.

In all such cases, the nature of the derangement of the physiological processes should be carefully considered before any plan of treatment is adopted. We must ascertain in what points the condition differs from a healthy state, and then consider how the deranged actions may be restored. In such cases, it is obviously quite useless to attempt to relieve the patient by giving drugs, without enforcing attention to all the circumstances which are likely to improve the health. Neither will it be wise to attempt to treat the case as if the presence of the uric acid deposit were the most important symptom. The physiological changes taking place in the organism generally must be attended to, and the patient must be instructed to employ those means which are likely to restore healthy action.—*British Med. Journal, Oct.* 22, 1859, *p.* 850.

46.—ON THE CHLORIDE OF SODIUM OF THE URINE.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

[The quantity of the inorganic salts in the urine varies greatly in disease. Deficiency may depend on a deficiency in the food. Excess in such as are formed in the organism by the disintegration of the organic tissues (as sulphates and phosphates) may depend on increased disintegration of these tissues.]

The fluctuations observed in the quantity of common salt excreted in the urine are very great even in health. The circumstances which affect the proportion of chloride of sodium are very numerous, and these are greatly increased in disease. It was found by Redtenbacher, many years since, that in pneumonia the quantity of chloride in the urine gradually decreased as the inflammation advanced; and that in many instances, when the lung became hepatised, not a trace could be detected in the urine. Some years ago, I determined quantitatively the amount of chloride in the urine from day to day in cases of acute pneumonia, and the following case illustrates very well the changes which occur in the urine in this affection.

The patient was a plasterer, aged 24, and was under the care of Dr. Budd. On the third day of the disease, there was dulness two inches below the left mamma in front and behind over the space below the spine of the scapula. Bronchial breathing and bronchophony were audible over the lower angle of the scapula. Expectoration viscid, frothy, and slightly rusty; pulse 144, small and weak; respirations, 52.

On the fourth day of the disease, bronchial breathing and bronchophony were more distinct. Pulse, 116; respirations, 28. He was treated with small doses of antimonial wine, and was put upon milk diet and beef-tea. Turpentine stupes were applied to the chest. He progressed favourably, was convalescent within three weeks after the commencement of the attack, and was discharged well in little more than four weeks.

13. *Fourth day* of the disease. Urine high coloured; acid; specific gravity, 1017; contained a little albumen.

14. *Fifth day.* Acid; specific gravity, 1013; natural colour.

15. *Sixth day.* Acid; specific gravity, 1016; pale; still contained a trace of albumen.

16. *Tenth day.* Acid; specific gravity, 1022; no albumen.

17. *Twenty-second day.* Acid; specific gravity, 1016; pale; no albumen.

Analysis.

	13		14	15	
Water	956·60		957·40	954·00	
Solid matter	43·40	100·00	42·60	46·00	100·00
Organic matter	40·28	92·82		44·64	97·05
Fixed salts	3·12	7·18		1·36	2·95
Chloride of sodium	0		traces	0	

	16		17	
Water	955·00		968·40	
Solid matter	45·00	100·00	31·60	100·00
Organic matter	42·12	93·64	23·26	73·61
Fixed salts	2·88	6·4	8·34	26·39
Chloride of sodium	0		4·56	14·43

The decrease of the fixed salts generally, during the stage of hepatisation is remarkable. The last analysis of the urine, when the patient was well, shows the healthy proportion. In vol. xxxv. of the 'Medico-Chirurgical Transactions' will be found several other cases showing similar results. In some of the cases, it was shown that, although there was not a trace of chloride in the urine, and the blood contained less than its normal proportion, the sputa were very rich in chloride of sodium. In one case, it amounted to upwards of 18 grains in 100 of the solid matter of the sputum. In a fatal case, much chloride was found in the products effused into the air-cells of the lung.

In most exudations, and in growing tissues, there is a considerable amount of chloride of sodium. In acute inflammations generally, the proportion of chloride in the urine gradually diminishes until the disease is at its height. When resolution occurs, the chloride reappears, and gradually increases as convalescence advances, until it attains its normal standard. The amount of chloride in the urine is much influenced by the nature of the food, and by the quantity of fluid taken, as remarked when considering the chloride in healthy urine; but the results above described cannot be explained in this manner; for although patients take less food when they are ill, and therefore less salt, the same results are observed if salt be given to them. Moreover, the disappearance is gradual, and the reappearance is marked by a change in the symptoms of the disease, although the food has remained the same during the whole period of the illness. I have expressed the opinion that, in all probability, the chloride is attracted from the blood in undue proportion to the point where the inflammatory changes are taking place; and that, instead of passing through the organism as it does in health, it accumulates at this point until a certain stage of the morbid process is passed, when it is reabsorbed into the blood, and excreted by the ordinary channel. The precise office which the salt plays in these processes is not understood; but certainly, in all the specimens of inflammatory lymph that I have examined, I have always found common salt present in large quantity. In many cases of bronchitis, acute rheumatism, pleurisy, in some cases of skin-disease, and in some other instances in which its absence would appear to be merely an accidental circumstance, no salt can be detected in the urine. We cannot, therefore, regard this diminished proportion or absence of chloride in the urine as a point of any value in the diagnosis of pneumonia, although it must be looked upon as a fact of great interest with reference to the morbid changes which are taking place at the time. The conclusions to which I arrived, after examining the urine, blood, sputum, and inflammatory products, in several cases of pneumonia, are as follows:

1. That in pneumonia there is a total absence of chloride of sodium from the urine at or about the period of hepatisation of the lung.

2. That, soon after resolution of the inflammation, the chloride is again present in the urine, and often in considerable quantity.

3. That, at this period (resolution), the serum of the blood is found to contain a greater amount of chloride than in health.

4. That the presence of chloride of sodium in the urine may be taken as evidence of the existence of a greater quantity of the salt in the blood than is required for the wants of the system generally, or, at least of an amount sufficient for that purpose, and that the absence of the salt from the urine indicates that the circulating fluid contains less than the normal quantity.

5. That the sputa in pneumonia contain a greater quantity of fixed

chloride than healthy pulmonary mucus, if there be not much less than a normal amount in the blood, although there be a complete absence of the salt from the urine. In all cases, however, there is found in the sputa a quantity many times greater than exists in an equal amount of blood at the same period of the disease. The absolute amount present is subject to variation at different periods of the disease, and in different cases.

6. That, in one case which was fatal, the proportion of chloride present in the sputum underwent a decrease, while the amount of solid matter, and especially the attractive matters, increased in quantity. At the same time, the sputum became acid; and in the matters expectorated within the last few hours of the patient's life, a large quantity of grape-sugar was found; but, in that obtained on the day previous to his death, none could be detected.

7. The absence of chloride of sodium from the urine during the stage of hepatisation seems to depend upon a determination of this salt to the inflamed lung; and, when resolution occurs, this force of attraction ceases, and whatever salt has been retained in the lung is reabsorbed, and appears in the urine as usual.—*Brit. Med. Journal, Nov.* 19, 1859, *p.* 941.

47.—ON THE PHOSPHATES OF THE URINE.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

Alkaline Phosphates.—Much has already been said upon the quantity and origin of the alkaline phosphate in the urine; and I have brought forward evidence to show that the greater part of the phosphoric acid eliminated is carried into the organism in the food. A certain proportion. however, there can be little doubt, is formed in the body by the oxidation of the phosphorus of albuminous textures (nervous tissue). In diseases generally, the alterations which have been observed in the quantity of phosphate removed in the urine is to be attributed, to some extent, to the altered diet of the patient. It is reasonable to suppose that, in some conditions of the system in which a more than usual disintegration of tissues rich in phosphorus takes place, more phosphoric acid is formed in the organism than in health. This excess should be found in the urine in the form of alkaline phosphate, and the amount ought to correspond to the activity of the changes taking place. By ascertaining the proportion, we should be able to form an estimate of the quantity of phosphorus oxidised, and therefore of the nerve-tissue disintegrated, of which it was a component part. The really difficult part of the inquiry is to ascertain how much of the total proportion of phosphate present is derived from the food, and how much is actually formed in the organism. The sulphuric acid is almost entirely produced in the body; and there is not, therefore, the same difficulty in estimating the amount of sulphur oxidised, as there is in the case of the phosphorus.

Of late, the importance of this subject has been much increased by attempts to advance the experimental results already obtained in favour of the hypothesis, that the amount of phosphate excreted in the urine is to be regarded as an index of the activity of the nervous system. Those who labour to prove that all the changes in the body are the direct result of certain chemical decompositions, have not hesitated to bring forward these results in favour of their theory. It seems by some to have been regarded as a settled point, that the quantity of phosphate in the urine varies according to the amount of nervous tissue disintegrated; and it has been assumed that the quantity of work done by the brain is in direct proportion to the activity of the chemical changes going on in the nervous tissue. This question is obviously a most important one, and much more is involved in it than at first appears. I propose, therefore, to mention a few of the facts which have been ascertained; and I think we shall find that, in this matter, speculation has taken the place of reasoning founded upon facts and experimental observations.

Dr. Bence Jones, as is well known, has written several important papers upon this subject. The general conclusions to which he has arrived are the following:—

"In delirium tremens, and in other delirium, a remarkable increase in the amount of sulphates in the urine was frequently observed; and the total phosphates were in the same cases occasionally remarkably diminished.

"In acute inflammatory diseases of the nervous structures, during the most febrile symptoms, an increase was observed in the amount of sulphates in the urine; and the total amount of earthy and alkaline phosphates in these diseases appeared to be increased in the same proportion as the sulphates were increased." ('Phil. Trans.,' 1850, p. 66.)

"In fractures of the skull, the phosphatic salts increase only when any inflammatory action occurs in the brain; and in acute phrenitis, an excessive increase takes place. In delirium tremens, there is a marked deficiency of phosphates, unless they are introduced with the ingesta; an excess is, however, met with in some functional affections of the brain."

These conclusion sare founded upon analyses of 1000 grains of urine, in eleven cases of delirium tremens and eight cases of acute inflammatory affections of the nervous centres.

[Some time since the author examined the urine of many patients in St. Luke's Hospital for Dr. Sutherland, with the view of ascertaining the proportion of alkaline phosphate excreted in different cases of mania, dementia, and paralysis of the insane, &c.]

The conclusions at which Dr. Sutherland arrives are the following:—

1. A plus quantity of phosphates exists in the urine in the paroxysms of acute mania.

2. A minus quantity exists in the stage of exhaustion in mania, in acute dementia, and in the third stage of paralysis of the insane.

3. The plus and minus quantities of phosphates in the urine correspond with the quantitative analysis of the brain and of the blood; for a plus quantity of phosphorus is found in the brain, and a slight excess of albumen in the blood of maniacal patients; and a minus quantity of phosphorus and albumen are found in the brains of idiots, and a minus quantity of albumen in paralysis of the insane.

4. The plus quantity of phosphates in the urine of cases of acute mania denotes the expenditure of nervous force, and is not a proof of the existence of acute inflammation in this disease.

[The author concludes this part of the subject by saying that, though not denying that increased nervous action may be associated with the formation of an increased quantity of phosphoric acid, which is eliminated in the urine, yet the facts hitherto advanced in favour of this view are by no means conclusive.]

Earthy Phosphates.—The proportion of earthy phosphates does not seem to vary much in disease. Dr. Bence Jones has shown that, in cases in which the alkaline phosphates are increased, there is no corresponding increase in the proportion of earthy phosphates. Much of the earthy phosphate eliminated in the urine in health is doubtless derived from the food, but a certain proportion is set free in the disintegration of the tissues, especially the osseous tissues. An increase of earthy phosphate is observed in the urine in some very rare cases of disease, in which the earthy matter of the bones is absorbed *(mollities ossium)*. In one acute case of this disease, Dr. Bence Jones ('Phil. Trans.,' 1848) obtained indistinct evidence of the presence of chlorine, and suggests that in future this substance should be searched for, as it may possibly be directly concerned in the removal of the earthy material from bones.—*British Med. Journal, Nov.* 26, 1859, *p.* 962.

48—CASE OF BRIGHT'S DISEASE,

IN WHICH PERMANENT RECOVERY TOOK PLACE, THE RESULTS BEING VERIFIED BY DISSECTION, THE PATIENT DYING OF OBSTRUCTION OF THE BOWELS EIGHT YEARS AFTERWARDS.

By Robert Scott Orr, M.D., Dispensary Physician to the Glasgow Royal Infirmary.

[The patient was a gardener, 57 years of age, thin, active, and of temperate habits. His illness commenced slowly and insidiously, with dropsy, but he rapidly grew worse, and presented the following symptoms:]

His face and eyelids were swollen and puffy. His lower limbs were hard and œdematous, and so great was the anasarca, that his thighs were swollen to the size of the body of a man of average bulk. His urine was frequently examined, and found to be highly coagulable, and of low specific gravity.

[He was seen also by Drs. James Watson and John Couper, of Glasgow, and his case considered almost hopeless. The treatment adopted]

Was the administration of mild diuretics, such as the acetate of squill and sweet spirit of nitre, the bitartrate of potass being given freely as a diet drink. He also had five grains of Dover's powder three times a-day, and an occasional warm bath. Latterly, the tincture of muriate of iron, with tonics, was found very beneficial.

[Under this treatment, in about three months, the graver symptoms subsided very gradually, and after a time the health was slowly restored, and recovery became complete and permanent. This patient lived for eight years longer in perfect health, and ultimately died from an intestinal affection. At the autopsy it was found that]

The right kidney was small, and in some parts deeply notched, its envelope being puckered in many places. The cortical part of it, on a section being made, was found very much narrowed and thinned. The tubular tufts were, some of them, very healthy-looking; others were much encroached upon by granular matter. The granules appeared to me much less in size and more compressed than as seen in ordinary cases of granular kidney, and as if a process of atrophy had been going on in them. Indeed, the whole kidney appeared to have become atrophied, although in this man's body all the viscera were found unusally small.

The left kidney could nowhere be found in its usual situation, but after diligent search was discovered lying on the sacrum, exactly in the fork formed by the bifurcation of the aorta. It was very much smaller than the right, having undergone much atrophy; it was nearly circular in form, presented the same notched and puckered appearance externally as the right, but contained more granular matter, of the same atrophied, compressed character as in the other. Both kidneys conveyed to my mind quite the impression that they had undergone a process of repair.

The liver and spleen were both much smaller than usual, but healthy in structure.

In the same year that I treated this person for albuminuria, apparently in its advanced stage, with the result above related, Professor Christison delivered two clinical lectures at Edinburgh on Bright's Disease, in which, with his usual clearness and perspicuity, he gave a very complete general sketch of what was then known regarding the disease. Under the head of "Treatment," he alludes specially, and at some length, to the curability of the malady. He says, "Bright's disease was long considered a very deadly one, indeed, as generally fatal sooner or later. This denunciation is probably true of the stearotic form. But it is quite incorrect if extended to the more frequent form of inflammatory desquamation." He then gives a selection of cures, where he had the rare advantage of observing the results for a very considerable term of years after the first apparent recovery. To those who maintain that there is no such thing as Bright's disease without dropsy, and who will, of course, deny that some of the cases he quotes were really instances of that disease, but only simple chronic nephritis, he replies, that he has seen cases precisely similar without anasarca, but fatal, in which after death the kidneys presented the characteristic appearances of advanced Bright's disease.

One of the cases with dropsical symptoms, I shall briefly quote here. The patient was a retired merchant in the country, who had been attacked with universal and extensive anasarca. He was sixty-three years of age, totally blind, nearly deaf, very intemperate, cross-tempered, &c. His surgeon had failed to detect any disease in his heart, lungs, or liver, and had not thought of his kidneys. Dr. C. found his urine of moderate density, rather pale, somewhat turbid, and pretty strongly coagulable. He directed digitalis, squill, and bitartrate of patass, to be given, to excite diuresis. These remedies had the desired effect; his urine acquired its healthy character, he perfectly recovered, and remained free of any urinary disease for seven years. He then again became Dr. Christison's patient for *gangrena senilis* of right foot, of which in about two months he died, without any return of anasarca, or of urinary symptoms of any sort. An examination of the body could not be obtained. Now, if any link was wanted to complete the chain of evidence as to a permanent cure having been obtained in this case, it would have been supplied by a post-mortem examination. To have seen and described the state of this man's kidneys, would have been most interesting, and probably conclusive. This link is, I think, supplied by the case which occurred to me, and which I have already detailed. The symptoms in my case were distinct and very severe, more so, I think, than in that of Dr. Christison; the general dropsy was very extensive indeed, and his urine was very highly coagulable; his distress was very great, and altogether he was considered, both by myself and the other medical gentlemen who saw him, to be in a most hopeless condition. Then the appearances on dissection, which were to me, after the lapse of so many years since his illness, intensely interesting—were to my mind quite conclusive that a process of repair had taken place in the kidneys. If, however, this be denied, it will, I think, be conceded that at all events the progress of the disease had been arrested, that atrophy of the organs as well as of the diseased structure had taken place, and that what was left of healthy structure had recovered its functions. To any one at all acquainted with the aspect of the granular kidney in its advanced stage, the appearances seen in the case described are quite different.

The effects of the disease, it is true, were there—the organs were atrophied—but they presented the natural pink colour, quite distinct from the sandy, pale, blanched appearance of the granular degeneration ; and, as the man's history proved after his recovery, they were perfectly competent to perform their functions.

It is very likely that Dr. Christison, or other observers, whose attention has been directed to the curability of Bright's disease, may, since the two very interesting lectures to which I have referred were delivered, have been able to verify by dissection cases where real recoveries of long standing had taken place. As, however, I have not been able to find any recorded cases, where, after the patients had presented unequivocal signs of the disease, they had recovered, remained well for a series of years, died of some other disease, and presented no signs of urinary disease during the last illness, and where the appearance of the kidneys was described after death—I have thought it right to record this as such a case. It affords additional confirmation of the reality of the fact that such recoveries do sometimes take place, and I beg to adduce it here in corroboration of the accuracy of the opinion enunciated by the very distinguished observer, whose lectures I have taken the liberty of referring to.

The case is, however, deficient in one very important particular; there is no microscopical report of the structure of the kidneys. This I regret extremely, as it would have rendered the case complete : but as it did not occur in town, there was no means of having a microscopic examination conducted by a thoroughly competent observer. It would have been very interesting to have ascertained what appearance the atrophied kidney presented, as well as the nature of the intimate structure of the compressed granular matter. It is not likely that any fatty matter existed, as this form of the disease is by all observers considered to be incurable. It is possible, however, that some reparatory process might have been detected, perhaps that described by Mr. Simon in the 'Medico-Chirurgical Transactions' (1847), viz., a cystic arrangement by which vesicles are substituted for the normal tubularity of the gland, and new secretory apparatus organized.

This case was originally intended for insertion in the 'Clinical Record,' and but for its length it would have been placed there. It affords encouragement, and should lead us not to despair of any patient, however bad his symptoms may be ; for if he be of temperate and regular habits, as in this instance, and the other organs be healthy, the disease in the kidneys may be arrested in its progress, and recovery may be the result. It should never be forgotten, that, as Dr. George Johnson remarks, "it is not precisely a disease *in* the kidney, but a constitutional disease, manifesting itself *at* the kidney," which we have to contend with.—*Glasgow Med. Journal, Oct.,* 1859, *p.* 263.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, AMPUTATIONS, &c.

49.—ON EXCISION OF THE KNEE-JOINT.

By Oliver Pemberton, Esq., Surgeon to the General Hospital, Sydenham College, Birmingham.

[One great question with regard to the operation of excision of the knee is the utility of the limb preserved; for if in any class of cases it should prove that the limb, even if preserved, is perfectly useless, and even an encumbrance, the advisability of the operation in such class of cases falls to the ground. In December, 1853, Mr. Pemberton removed the left knee-joint of a pale strumous lad, aged 12, for extensive disease of the joint. The following is the report of the state of the patient in October, 1859, *i. e.*, six years after the operation:—]

The patient came to my house. He had grown in height, and had thickened considerably in his figure, but was somewhat diminutive for his age (18). The lower limbs presented a wonderful contrast in appearance. The one was strong, with the muscles, bones, and joints well defined; the other, feeble and blighted. The sound limb, from the anterior superior spinous process of the ilium to the outer malleolus, measured thirty-four inches; the one subjected to operation, twenty-five! Thus showing a difference of nine inches: or a deficiency in growth as compared with the other of rather more than five inches since the operation. In the middle of the thigh, in the lower third, and round the calf of the leg, the measurements were sixteen, twelve, and twelve as opposed to thirteen, ten, and nine and a half. The foot of the limb operated on appeared to have expanded most, but even this was an inch and a half shorter than its fellow. The femur, the tibia, and the fibula of this limb were not larger than the same bones in a child of ten or twelve.

About the excised joint, the parts were sound, and free from pain on manipulation, a very moveable ligamentous or fibrous medium connecting the articular extremities. Notwithstanding the shortness and the flail-like joint, it was astonishing to see the power he possessed of extending the leg, and of bearing the entire weight of the body on it in walking, unaided by support of any kind; and it was quite clear that the disparity in length alone prevented him from realising all the advantages that he might under other circumstances have obtained from the operation. With all these drawbacks, he works hard as a boatbuilder, the limb being aided by a cork sole of some six or seven inches in height, and by a leather case at the knee.

The limb cannot, however, be deemed otherwise than an incumbrance. and with the best appliances to remedy the want of length, proving, after all, little better than a sad deformity.

In 1855, an authority on excision of the knee thus expresses his opinions on this question of subsequent growth.

"Another objection has been brought forward against the operation of excision, and which demands grave consideration. It has been asserted, that where the operation is performed in early life, the growth of the limb has been checked, and, consequently, that the lower extremities do not keep pace with each other, and hence one becomes so disproportioned to the other as to be ultimately useless." (Butcher, 'On Excision of the Knee-Joint,' First Memoir, p. 46).

In 1857, the same writer reiterates these sentiments. "In my former essay I proved, by several instances, that the growth of the limb was not checked by excision of the joint in childhood; and am happy now again to confirm this most important fact." (2nd Memoir, p. 60.)

Speaking of excision of the knee, Mr. Syme writes: "I tried the operation nearly twenty years ago, on a boy, who perfectly recovered from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But, in the course of time, it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation gradually diminished in respective length, until it wanted several inches of reaching the ground when the patient stood erect." (Pathology and Practice of Surgery,' p. 225: 1848.)

I have quoted the preceding remarks word for word, as they commence and end, in the eighteenth article of Mr. Syme's work, in preference to alluding to his mere opinion on the matter; for the same paragraph, in all its identity, has been frequently referred to and cavilled at during the past nine years.

Unhappily, in the case I have narrated, I fear that another "existing proof" of the fact *is* added to the only one that Mr. Butcher (First Memoir, p. 46) could discover, when he wrote his first and second memoirs on excision of the knee, that the growth of the limb is at least sometimes checked when the operation is performed in early life. This most undaunted surgeon has, I fear, been led to adopt those conclusions on the question which his wishes and anticipations too clearly held in view. He does not write from his own experience, but strengthens the position he assumes by the communicated observations, amongst others, of Dr. Keith of Aberdeen, Mr. Page of Carlisle, and Mr. Jones of Jersey.

Dr. Keith's case was a boy, aged 9, from whose knee two inches of bone in the whole, about equal portions from the femur and tibia, were removed; three years afterwards, the limb was reported as "plump, and growing as fast as the sound limb." There was firm bony anchylosis.

Mr. Page's patient was a young lad, aged 17. Two inches and a half of the femur and tibia were removed. There was firm bony anchylosis. Reporting on his condition four years afterwards, he writes: "the growth of the stiff limb has quite kept pace with that of its fellow."

Mr. Jones's observations refer to the cases of three boys, aged respectively 7, 11, and 9. In the first instance, two inches and a quarter of bone were removed. There was bony anchylosis. In the second, rather more than four inches of the femur and tibia were removed. There was bony anchylosis. In the third, the exact measurement of bone removed is not mentioned; but there was bony anchylosis. This patient died at the end of twelve months; and Mr. Jones writes: "There cannot be a doubt that in this instance the limb, from which the joint was excised, kept pace with the other in every respect." Whilst in regard to the first mentioned cases no disparity of growth is hinted at.

From Dr. Keith, the distinguished surgeon of Aberdeen, I have received the following most lucid report of the present state of the boy's limb, on whom he operated nearly six years since. Dr. Keith's letter is dated October 12th, 1859; and he thus writes to me:—

"Dear Sir,—I have pleasure in replying to your favour of the 7th current, but have not the additional pleasure of reporting so favourably of John Hay's limb, as I seem to have felt warranted in doing three years ago.

"Be the cause what it may—and I shall be delighted to have your opinion on the subject—I, and I presume every one who has had experience in resection of the knee, feel convinced that growth to the length is all but arrested in the lower limb after every such operation.

"I delayed my answer until I should have opportunity to see and to measure John Hay's lower extremities; this I accomplished yesterday, and annex the result.

"He is now a stout healthy boy, in his fifteenth year; his left or sound lower limb long and well developed. His right limb is really plump to look at, but seems a mere appendage to his body, when compared with the other limb; the right limb being now *seven inches* shorter than the left limb, from the anterior superior spinous process of the ilium to the heel.

"At the thick of the thigh the left measures 17 inches round; the right, 16. Around the knee and patella: left, 12½ inches; right, 12. Around the calf of the leg: left, 11½ inches; right, 10½.

"In length, the left limb is 17 inches from the anterior superior spinous process of the ilium to the centre of the patella, and 17 inches from the same spot to the sole of the foot, in the whole 34 inches; while the right was only 12½ to 14½; total, 27 inches.

"Yet, wearing a boot propped up by two steel rods seven inches long, he walks and runs with great ease and energy; and when I yesterday asked him, If he now wished the limb had been removed, he at once exclaimed, 'Oh, no! my leg is worth a thousand wooden ones; it is my own leg, sir, and I feel it to be so.' I daresay the poor fellow is right. The arrest of growth to the length is a mystery. Let me see that you solve it.

"To account for my statement made to Mr. Butcher three years ago, it is only necessary to state that exercise had developed the muscles so, that the limb was plump; he wore the same high-heeled boots that I had at first provided for him, so that no increase of disproportion in length had at that date taken place between the two limbs; but now my guess is, that the dwarfish little fellow had been standing still, and neither limb growing in length, it was only when he began to shoot out in earnest that the difference became apparent, and new boots with higher and higher props became the order of the day.

"Hay's right limb has an outward curve at the knee now, though anchylosis is perfect, and he thinks it is slowly increasing. That tendency from the hour of the operation has been often noted. How is it to be obviated?

"1. I think well of cutting the tendons forming the inner hamstring.

"2. Of slicing rather more off the inner than the outer condyle of the femur.

"3. Using a splinted knee-cap for life.

"The patella was spared in this case, and has strong union to both femur and tibia; and yet with all this the callus yields after five years."

The foregoing narrative of Dr. Keith will be attentively perused by practical surgeons. In two respects, his case possessed an advantage over mine. There was anchylosis; and but half the quantity of bone was removed. Notwithstanding, at the end of nearly six years, the deficiency of growth is measured by five inches—the same amount that was recorded in my case, and extending over the same period of observation, within three months.

The circumstances which led Dr. Keith to state, in the first instance that the limb was "growing as fast as the sound limb," are, in my judgment, sufficiently explained by the presumption that neither limb had then grown, or, if they had done so, it was only in a very limited degree. I had the same impression some two or three years after I had operated on E. F.,; but I found subsequently that the real stride in development had not then commenced; and that, when it did, the limb subjected to operation did not partake in its advance.

The lateral yielding of the callus is not, in my experience, of frequent occurrence, where firm bony union has taken place. It may happen in young subjects, and, if so, forms another drawback to the proceeding being adopted in early years; but it certainly does not occur in adults. I should, however, always use in the ungrown limb a splinted knee-cap, as Dr. Keith suggests, to guard against the possibility of the event.

The next case, which was selected to illustrate the parity of growth, was under the care of Mr. Page. But the patient was 17 years of age; and thus being past puberty, his condition cannot fairly be adduced in support of the theory.

Then we have Mr. Jones's three boys. The third boy, aged 9, died twelve months after the proceeding. His limb was found to have "kept pace with the other in every respect"—a statement after what we have now ascertained, that cannot amount to any determination of the question at issue. The first and second boys, however, in whom large quantities of bone were removed, should now have their limbs reported on, as, from the length of time that has elapsed since the operations (eight and seven years), the evidence which their subsequent history could afford would be most important.

Being anxious to know about these cases, I wrote to Mr. Jones concerning them; but my letter arrived at Jersey at the time of the calamitous fire in the General Hospital, or I doubt not that I should have received the information I sought. As it is, I trust that the profession, through some other channel, will ere long be in possession of all the facts concerning their present state, which this most able surgeon can afford.

On what, let us inquire, when the operation of excision of the knee is performed in early life, and the subsequent growth of the limb does not correspond with its fellow—on what does this depend? Mr. Humphry says, "In young persons, care should be taken to make the section through the epiphyses of the tibia and femur; so that a thin layer of the epiphysis, with the cartilaginous medium that unites it with the shaft, is left upon each bone. If this precaution be taken, there is every reason to believe that the limb will keep pace in its growth with the opposite member." ('Med.-Chir. Trans.,' vol. xli. p. 216). This opinion of so distinguished an authority on the development of the skeleton as Mr. Humphry, is entitled to every consideration, and, at first view, it appears plausible; but it is not borne out by facts.

Mr. Price writes: "Should the epiphyses of the bones be removed entirely, there can exist but slight doubt that arrest of further development will take place." ('Med. Times and Gazette,' April 23, 1859). If this were so, there ought to have been an arrest in the growth of Mr. Jones's first and second instances, in which the bones must have been sawn through beyond their cartilaginous joinings, as well as in the boy operated on by Mr. Page, at whose age (nine) an inch from either articular extremity could scarcely fail to go beyond the epiphysis.

On the other hand, Mr. Syme informs me (July 30, 1859), that the articulating surfaces only were removed in the instance from which he drew the conclusions already cited, the bulging parts of the bones being left.

But, supposing that the bones in the thigh and leg do not grow when deprived of their heads. how are we to explain the want of growth in the case I have narrated, in the foot and the fibula, in the absence of the plea of want of use, inasmuch as the boy has worked his little limb for years with persevering activity?

That the theory of growth being arrested in young subjects by the removal of the heads of bones, is an untenable one, we have other facts in surgical experience to prove. Take, for illustration, amputation in the lower third of the thigh or leg, or at the ankle-joint. I have never found an arrest in the subsequent growth of the bones in my experirience, in the first class of cases; and I am not aware of others having met with a different result. The complaint has been, rather, that the bone or bones in the stump become troublesome, and grow too much, so as even to require the removal of some inches at a period of years very distant from the operation. At the ankle-joint, I have especially watched the parity of growth. I had occasion to amputate at this articulation in an infant only twelve months of age, nearly six years since. The malleolar epiphyses were removed. The subsequent growth of the limb has been of the most perfect character—a condition invariably observed in several other instances where I have been called on to perform this operation at tender years.

That the limb may grow in the young subject after excision of the knee, the evidence which has been given me on the subject by my friend Mr. Humphry goes some way to prove, but not, from the age of the patient at this time, quite far enough to establish an accomplished fact, oven in his well observed cases; but we must at the same time acknowledge that it may not do so—a conclusion that cannot fail to make us desire a further inquiry into the causes of failure in this respect, ere we determine to recommend the adoption of the operation in children generally,

The late Mr. Mackenzie hazarded the opinion that the want of anchylosis might exercise an influence on the subsequent growth of the limb. ('Monthly Journal of Medical Science,' June 1856.) He based it on Mr. Syme's case, in which the limb allowed a slight degree of flexion and extension; and, considering that my case afforded even greater mobility, the conjecture, but for Dr. Keith's illustration to the contrary, might have deemed to be not unfounded.

It will be seen, however, that on the two points—the section through the epiphyses, and the union by anchylosis—Mr, Humphry's case is very conclusive in an argument of results. This able surgeon thus writes to me, July 22nd, 1859:—

"The only case I know of in which sufficient time has elapsed to enable me to derive satisfactory information respecting the growth of the limb after excision of the knee, is that of William Child, related at p. 196 of the last volume of the 'Medico-Chirurgical Transactions.'

The operation was performed in August 1855, when he was twelve years old: and in two months there was firm union between the bones. In consequence of your note, I drove over to examine the lad yesterday, and find that, during the interval of four years, the growth of the limb has proceeded *pari passu* with that of the other limb and of the rest of the body. The bones of the foot are quite as long as those of the other side—the tibia and fibula very nearly; certainly showing no greater difference than is due to the removal of a thin slice of the upper articular surface of the former. The femur is about an inch shorter than the other femur. The difference in this respect between the femur and the tibia, is I suppose owing to the greater thickness of the slice removed from the condyles of the femur, in order to obtain a flat surface for union with the tibia."

A fair consideration, then, of the foregoing facts, leads us to certain conclusions affecting the operation of excision of the knee in young subjects. These are:—

Firstly. That the proceeding is liable to be attended by a want of corresponding growth in the limb subjected to operation, as compared with its fellow; the result being, that ultimately the member becomes not only useless, but is an incumbrance, as it has failed altogether to grow in proportion to the general expansion of the frame.

Secondly. That whilst we, at present, have no proof that the removal of the epiphyses, or the failure in obtaining anchylosis, exercise any material influence on the subsequent growth of the limb, it nevertheless appears probable that adequate growth is more likely to be attained where care has been taken to remove as small a portion of the articular extremities as possible, and anchylosis has resulted.

Having testified to the truth, so far as my own experience can guide me, on this deeply interesting question, as it affects the operation in early years, I shall now rest satisfied, trusting in the hope that we may yet reach the means whereby even this cause of failure may be removed, as time enables us to know more and more the results that may have been realised by others.

[Although the circumstances which necessitate amputation are also those which indicate excision, yet the particular operation cannot be chosen indiscriminately in all cases, for each of these operations possesses certain advantages over the other.]

In chronic cases of diseased articulations requiring an operation, the state of the bones and soft parts must be attentively considered.

Those cases will be found most eligible for excision where the mischief in the bone is limited to the articular extremity, and where the soft parts have not become adherent to the parts beneath, from the presence of sinuses, or from the general effects of long-continued disease.

Where the disease in the bone cannot be removed without the sacrifice of too great an amount, or where the soft parts around the joint have assumed an unhealthy character, amputation should be preferred; a vigorous condition of the latter structures being quite as essential to subsequent repair and well-doing as is a section through sound bone for the attainment of satisfactory osseous or fibrous union. Cases in which there has occurred the most complete alteration of the synovial membranes, accompanied by destruction of the cartilages, together with the ligamentous tissues, so long as the bones and soft parts are affected within the limits laid down, will be equally suitable to excision.

In forming our judgment, the constitutional power of the patient must not be disregarded. There are many instances of knee-diseases, requiring an operation, in which the pulmonary tubercular tendency is also manifest. The disposition to mischief in the lungs in these cases renders them, in my mind, ineligible for excision—in fact, unable to bear prolonged suppuration; whilst the removal of the disease by amputation, followed by the rapid recovery usual in such persons after such an operation, is well known to be attended by the happiest results.

In acute cases of diseased articulations, with suppuration, the selection of excision must be attended with great hazard. The occasions on which this has been done are very few. Free incisions into the cavity of the joint should be made, in the hope of thus relieving the disorder and obtaining anchylosis, in preference. The constitutional powers not admitting of this treatment, it will be better, then, to recommend amputation rather than resort to excision.

For deformity, and for accident, we may be also called on to recommend excision.

In the first class of cases there are many well suited to the proceeding. Among these are rectangular anchyloses, or where dislocation of the tibia backwards has occurred as the result of former disease. Such cases afford every prospect of an useful limb being obtained in the place of one not only a deformity, but an incumbrance.

In the second, at present, our experience is very limited. Mr. Canton, in the Charing Cross Hospital, performed the operation ('Lancet,' August, 1858) in a boy aged 8, who had received an injury to the lower end of the femur, whereby the epiphysis had been separated from the shaft. The case did well. My friend, Mr. Lakin, ('Midland Quart. Jour.,' Jan. 1858, p. 37), late assistant-surgeon to the General Hospital in Camp, before Sebastopol, also resorted to the proceeding in the case of a private of the 77th Regiment, who was wounded by a musket-ball which had penetrated the inner condyle. The case did well for twenty-six days, and then sank from diarrhœa and exhaustion. This was the only case that occurred in the Crimea.

The advantages of this proceeding in cases of accident must be, therefore, at present ranked as conjectural; but there would appear to be no valid reason why a primary operation of this character should not succeed in removing the impending danger of a compound fracture or laceration of the knee-joint as thoroughly as an amputation through the thigh.

The character of the medium binding the sawn articular extremities together, in the most favourable cases, will be found to vary very much. In some cases, bony union does not seem attainable by any amount of care and quietude; in others, it is as rapid as unexpected. Whilst, therefore, we may devote all our attention to the securing anchylosis, if possible, as promising, under all circumstances, the most serviceable limb, we need not be disappointed at there being a limited movement allowed between the bones when the cure is complete, as, in many instances where such a condition has been observed, the limbs have been, in their utility and power of endurance of fatigue, everything that could be desired; the only precaution to be kept in view being the necessity there is for wearing, under such circumstances, a splinted knee-case.—*Brit. Med. Journal, Nov.* 26, *and Dec.* 10, 1859, *pp.* 959, 998.

51.—AMPUTATION AT THE KNEE-JOINT.

(Case under the care of Prof. Fergusson, King's College Hospital.)

[Although noticed by a few of our metropolitan surgeons, the operation of amputation at the knee-joint has been somewhat overlooked. In nearly all the cases in which the leg has been thus removed, the patients have recovered with stumps of the most useful and serviceable kind, much better adapted to support the weight of the body by proper appliances, than when the stump exists in the thigh itself. The manner in which the operation is best performed is matter for consideration.]

It was customary at one period to make a large posterior flap from the fleshy part of the gastrocnemius, and to saw off a slice of the inferior surface of the condyles; and although in some cases the stump healed kindly, and was a good one, as the operation came to be repeated, it was deemed preferable to dispense with the large posterior flap, and also to leave the femur untouched. Mr. Lane, of St. Mary's, makes a large anterior cutaneous flap in front of the joint right down to the insertion of the ligamentum patellæ, which is then reflected upwards, the knife being carried through the joint. A small posterior flap is then made, so that in the process of healing it shall draw the anterior flap over the articular surface. The patella is retained, and forms a round and smooth surface to rest upon, as in the flexed knee, the skin being likewise movable over it, as in the healthy joint. The advantages of this method of performing the operation we have already adverted to.

In the subject of the following case, the notes of which were kindly furnished by Mr. Francis Mason, house-surgeon to the hospital, Mr. Fergusson made flaps of equal length, which met below the condyles, the patella being removed, and none of the articular surface being taken away. However favourable the case seemed to be, we regret to say it ended badly, owing, no doubt, to the irritable condition of the man's nervous system, induced by anxiety and restlessness. Non-success in this instance, however, cannot detract from the merits of this special form of amputation.

A man, aged forty-eight, was admitted, in October last, with a large tumour on the right leg. It appears that he had disease of the fibula for two years, and about eight months ago he was a patient in the hospital, when a circumscribed growth, as large as a fist, was present over the fibula, and was not then thought to be malignant. Mr. Fergusson, at that time, proposed the removal of the fibula itself, with the tumour, which appeared to involve the whole thickness of the bone; but the patient would not consent, and he left the hospital. For the last year the man has been very anxious to have something done, but he was very undecided. Lately he showed himself, with the tumour enormously increased in size, fully as large as an adult's head, and involving all the surrounding tissues, both in the front and back part of the leg, so much so that it became impossible to relieve him of it by any partial operation. The tibia still appeared to be healthy, yet the nature of the swelling was somewhat doubtful from its extreme hardness, as the muscles and aponeuroses were stretched over it, giving it the feel of an osteo-sarcomatous growth. The rapidity of its enlargement, however, seemed to show its malignant character, and there was no pain. The inguinal glands were slightly enlarged. Amputation was consented to; and, on the 15th of October, the leg was removed at the knee-joint, the man being fully under the influence of chloroform; and not an ounce of blood was lost.

In some observations made after the operation, Mr. Fergusson remarked, that this was a case in which it was necessary to remove the whole of the leg. Amputation at the lower third of thigh would have been the usual operative proceeding; but he thought he would resort to one less formidable—namely, amputation *at* the knee-joint. In his early operations, he removed the leg at the knee, and then took away a slice from the condyles of the femur; and hitherto the posterior flap had been made from the calf of the leg. Some surgeons, he said, have made a long anterior flap from the skin alone to cover the ends of the bone. This he considered somewhat opposed to the usual rules of surgery; yet in all this, there is a certain amount of novelty which ought to be investigated by the profession at large. He has had occasion, during the last fifteen years, to admire the stumps made *at* the knee in preference to those in the thigh; and referred to the case of a man, having a stump of this kind, who walked ninety miles in three days without inconvenience.

In making his flaps, Mr. Fergusson found the anterior one to be rather short, and the posterior one somewhat retracted. This was owing to the presence of the patella, which he considered to be drawn upwards. It was, therefore, removed, and the flaps were well brought together beneath the articulating surface of the femur, none of the bone being removed.

The patient went on well immediately after the operation. The wound, however, soon fell into an ashy condition, and subsequently a fatal result ensued, which may have been much influenced by the constant mental anxiety evinced by the patient for many months before the operation. The tumour was found to be medullary cancer, springing from the shaft of the fibula.—*Lancet, Dec.* 10, 1859, *p.* 586.

52.—ON EXCISION OF THE KNEE-JOINT.

By Eben Watson, M.D., Surgeon to the Royal Infirmary, Glasgow.

[Dr. Watson commences his paper with some tables and details of the more important of the cases operated upon by him, or by other surgeons in Glasgow, so far as he has access to know them. It seems, that the knee-joint has been excised eleven times in Glasgow upon the living subject. Of these cases only four have been successful; of the remainder four died from the effects of the operation; two after secondary amputation; and one after secondary amputation recovered. The pathological condition in these cases were very different. In a large proportion the synovial membrane was in that pulpy state, so commonly the result of chronic inflammation in a scrofulous subject. In many, the cartilages were likewise affected. In some pus had formed in the joint, and in a few the extremities of the bones were carious. With regard to the selection of cases, the author observes :]

The one important rule in all excisions is, that the diseased parts be *wholly* removed; and this can obviously be accomplished in all such cases as above alluded to, unless the bones be very extensively affected. There is, indeed, an apparent exception to this general statement, inasmuch as the whole of the synovial membrane cannot be removed, when the knee-joint is excised. Everybody knows that the large bursa above the knee-joint is lined by a synovial membrane, which is continuous with that of the joint; and, doubtless, the former is sometimes affected with pulpy degeneration, as well as the latter. But it is fortunate for surgeons that it is very seldom so severely affected; and at all events, after excision of the joint, if other things go on well, the disease in the bursa will not long or seriously hinder the favourable termination of the case.

It is different with periosteal abscesses in the thigh connected with bare bone. From all that I have seen of such abscesses, I should consider them a positive counter-indication to the performance of excision, should they unhappily be present in a case otherwise calling for the operation. It would seem, however, that the actual presence of pus in the joint should not be a bar to excision; for in several of our cases that did not prevent recovery.

In determining upon the propriety of performing this operation in any case, the general health of the patient ought to be carefully considered; and if it is not pretty good, excision should not be recommended. We should always bear in mind that patients subjected to this operation must be able to endure a long confinement to the recumbent posture; and their health must all the time be in such a state as will maintain a proper action in the wound. The great danger to be feared during this period, would seem to arise from irritative fever, which, when it sets in, soon reduces the patient to an almost or altogether hopeless state. I need hardly say, that if the tubercular diathesis is very strong at the time of the operation, it will co-operate with the surgical lesion to aggravate the danger referred to. This, then, should be considered very disadvantageous to the operation, and looked upon as calling for delay, or the adoption of amputation at once. Again, if there is active disease going on of any kind whatever in any of the internal organs, that should be a cause of delay, or, perhaps, a complete counter-indication of the performance of excision.

There is little doubt that some derangement of the general health, manifested very generally by disorder of the digestive organs, precedes that change in the wound which accompanies, or, perhaps, proximately causes the irritative fever. A liability to diarrhœa, or to sickness and vomiting, would also, therefore, be unfavourable, and should, if possible, be corrected before the operation is attempted,

A doubt was at one time entertained by surgeons as to the propriety of undertaking this operation in young and growing subjects; from the fear that the limb operated on would not grow correspondingly with its fellow. This, however, has been disproved by experience, and need not any longer occasion hesitation.

Operation.—Many methods of operating have been used in the performance of excision of the knee-joint. The oldest, and that which many still prefer, is to make an H incision; the patella being included in the upper flap, and the sides of the H being close upon and parallel with the hamstrings. This answers very well, but the wound is unnecessarily large. Another mode of operating is, by a single semilunar incision, commencing on the one side of the joint, and terminating on the other. This incision is generally made to pass below the patella, but Dr. Andrew Buchanan has always drawn it above the patella; and certainly this latter form of incision has the advantage of freely opening the bursa above the knee, and of avoiding the patellar ligament. It seems to me, however, that this likewise is an unnecessarily large wound.

I have lately operated by a single straight incision drawn right across from the one side to the other of the knee. Its middle corresponds with the middle of the patella, which I cut out, and open the joint. This is a much smaller wound than that occasioned by either of the other two modes of operating; and it is quite sufficient for the purpose required, namely, of fairly exposing the articular ends of the bones.

The removal of the patella is a recent improvement in the performance of this operation. I adopted it for the first time in one case before I noticed in the journals that others had done so before me. Mr. Fergusson of London has especially recommended this proceeding, because he has, in several cases, found it diseased long after the excision had been performed; and he had actually been obliged, in such cases, to cut out the bone, before the sinuses which it had caused would heal. In all cases it must retard the process of union between the femur and tibia; for, being placed directly above the uniting parts, it causes a constant dribbling of pus upon them, which must be disadvantageous. Lastly, should the patient get over these difficulties, and should the bone remain in front of the anchylosed joint,it cannot be of the least use; for it is required neither to strengthen the limb, nor to facilitate the action of its extensor muscles.

In cutting the bones I now use Graham's saw, and slant its blade so as to cut the end of the former convex, and thus save its length while I remove its entire surface. Again, I cut the head of the tibia, so as to make a concavity into which the convex end of the femur fits.

Any of the morbid structures which have not previously been removed. should now be cut off; and finally the knife should be drawn downwards at each extremity of the wound, so as to facilitate the escape of the discharge. The limb in most cases may now be straightened, and a few wire sutures inserted in the front of the incision. No bloodvessels of importance are cut in the performance of this operation, so that ligatures are seldom required, and almost no blood is lost.

One other point of importance belonging to the operation has occasioned some difference of opinion amongst surgeons; and that is whether or not the hamstring tendons should be cut, when from long disease they have become contracted, or whether they should be gradually extended afterwards. Now, our experience at the Royal Infirmary shows, that the latter process gives far too much pain, and interferes too importantly with the granulation of the wound, to be received as the proper practice. In one case, the worst consequences were attributed by the attending surgeons to the adoption of this proceeding. On the other hand, the cutting of these tendons already exposed at each side of the wound, could not do any harm, but on the contrary, would make the future management of the limb a matter of comparative ease to both patient and surgeon.

Dressing and Management after Excision.—This brings us to the consideration of the future conduct of the case, and, first, as to local management:—

I think most surgeons have agreed in the propriety of fixing up the limb in the position which it is to retain, before the patient has been removed from the operating table, and before he has recovered from the anæsthetic action of the chloroform.

What kind of splint is to be used? Mr. Butcher employs what he calls a fracture box fitted to the limb and well padded. The sides of this box turn down, and permit the dressing of the wound without moving the limb to any great extent.

Mr. Fergusson seems to employ a posterior splint and a long straight Liston's splint outside. Others have been contented with Macintyre's splint alone, or combined with the straight Liston's thigh-splint.

I have found it best to have no dressings applied to the limb at all, except a bandage round the foot and footpiece, pads of cotton, and water-dressings on the wound; so that, with these exceptions, the limb lies naked in the splint. It may thus be kept quite clean, and has enough of support for the first three or four weeks. About that time the limb becomes flabby, and the granulations often prurient, so that the bandage may with advantage be continued up the thigh from the foot. By this time the discharge will have greatly diminished, and the patient will be quite able to bear the necessary moving of the limb in applying the bandage.

I have also found the splint above described better than any other in another respect: viz., that it can be very easily taken off and exchanged or washed. If the tapes are all loosed, the side bands may be pressed back, so that the limb is then on a flat surface, from which the surgeon may lift it, without such difficulty as if it were in a box or a Macintyre's splint.

It is of the greatest consequence that the back part of the knee be well supported after excision, otherwise dislocation is apt to occur from the ends of the bones falling backwards. Now, there is nothing to prevent this in Macintyre's splint, which, when straight, has a vacant space in the position referred to. But in the splint which I recommend there is a flat and even surface all the way down the limb, on which, when properly padded, it lies equally supported at all points; and I think that no practical surgeon will undervalue the circumstance of the lightness of the apparatus by which these results are accomplished; for he knows that the weight of an apparatus, applied for any length of time to a patient confined to bed, increases his discomfort, as well as the danger of bed-sores.

This apparatus should be kept applied till the wound has healed, and till the patient can bear the limb to be moved without pain. I would not recommend the bony union of the tibia and femur to be very severely tested at this date; but I would exchange the apparatus for a shorter posterior splint of gutta-percha, reaching from the calf of the leg to the middle of the thigh, and applied with a roller-bandage. By and bye the patient may be allowed to rise and walk on crutches; but he should be carefully assisted at first, lest he fall and injure the affected limb, which might produce even then a fatal relapse.—*Glasgow Med. Journal, Oct.* 1859, *p.* 276.

53.—ON A RECENT CASE OF RESECTION OF THE KNEE-JOINT.

By H. GRAINGER EARNSHAW, Esq., M.R.C.S.E., Senior House Surgeon to the Liverpool Northern Hospital.

[The writer commences by stating his conviction that this case, though unsuccessful, offers much encouragement to the more frequent performance of the operation of excision of the knee-joint. The patient—an Irishman, forty-four years of age—was admitted a few minutes after receiving a severe injury to the knee, from a fall of ten or twelve feet.]

The immediate consequences of the fall to the knee were—1, a semi-elliptical wound, almost as if incised, commencing at the tubercle of the tibia, and continued upwards and backwards to corresponding points on the lateral surfaces of the joint, somewhere about its centre; 2, a jagged and total division of the ligamentum patellæ; 3, comminution of the lower part of the patellæ; and, 4, when the limb was purposely flexed, exposure of the whole interior of the joint.

At a consultation held three hours afterwards, preference was unanimously given to resection of the joint, rather than to amputation above it. Mr. Chalmers, who had charge of the case, accordingly operated at once. The flap was already half formed, and only required lengthening backwards. About an inch was removed from the extremity of the femur, and a very thin slice was sawn off the apposed surface of the tibia; the patella, with its fragments, was dissected out, and the slightly-lacerated edges of the flap pared. The ends of the bones were then applied to one another, and the limb was laid and secured in a straight, hollow splint (Liston's Macintyre's); and after the flap had been united to the integument below by a single metallic suture, simple wet lint was placed over the knee-part, and the man was then carried to bed, and carefully adjusted on air and water-pillows.

Chloroform was administered for the operation. Very little blood was lost, and only three small arteries required tying. The shock of the operation was of the very slightest amount. A little while after the operation, the patient had a forty-minim dose of Battley's solution, and this was repeated in the course of the afternoon, and again during the night. Water was constantly dripped on the lint, and milk and water only allowed for drink.

[The case is reported daily—progressing very favourably up to the 17th day after operation—when it is observed that the appetite, hitherto excellent, failed. Next day, Nov. 1st, the report continues:]

Seems not so well, but makes no particular complaint; his countenance is less bright, and there is a sensible alteration in its expression—from what cause cannot be made out; the discharge from the wound continues unabated.

From this time until the following Tuesday (the 8th) he gradually changed for the worse. About the 4th, it was suspected, in consequence of the change, and of slight fluctuation thereabouts, that matter was accumulating in the upper third of the thigh; still, there was no swelling or tension, and no heat or pain.

8th. There is now no doubt about pus being collected in the upper part of the thigh. A deep opening was made, and a pint and a half of thick yellow matter, containing lumps of disorganized cellular tissue, let out; and it is fully anticipated that there is a direct communication between this cavity and the knee.

Between the 8th of November and the 20th, the man's condition and prospect of recovery varied a good deal. At one time he was better in spirits and appetite; at another, he was worse again. All the time, the amount of discharge from the opening in the thigh diminished, while the amount and healthiness of that at the knee remained the same. On the 18th, the limb was for the first time taken out of the splint, there being evidently some tolerable union betwixt the femur and the tibia, and the whole length of it was placed on a water-pillow, secured in a straight position by cushions on either side and by a footboard at the foot. On the 20th, he was shivering, vomited frequently, and complained of intense pain in the opposite knee; upon examination of which there was no swelling, and but little pain on pressure. He now began to sink, and died on the night of the 23rd November, forty-one days after the operation.

Post-mortem Examination.—The whole flap had become soundly adherent, except for a small space at the outer and inner angles, whence the discharge escaped. The tendon of the rectus above the patella had also become firmly joined by fibro-cartilage to that below, and acted as a strong ligament between the femur and the tibia. After further dissection, the line of suture was seen to be concealed all around by a thin periosteal capsule. This was torn across by bending the leg, and in doing it the amount of direct union between the bones was measured by the reluctance with which they separated. For the eighth of an inch in depth from the circumference their tenacity was considerable; beyond this, to the centre, there was no adhesion. Also, on examining the two surfaces, around the borders was found fresh osseous matter, which could be scraped off and cut in fibres, and which grated against the knife; but it did not exist at all in the centre. The middle portions of both bones were of a deep-red colour, and easily penetrable by the point of the scalpel. The ossification commenced at the circumference, and was proceeding to the centre. The anchylosing process seemed to have been going on altogether prosperously. There was no swelling of the soft structures around the knee, nor had they a large suppurating surface underneath to explain the source of the discharge which had been continually, though moderately, given out from the outer angle of the flap since the eighth day of the operation. The cause of this was explained by laying open the thigh, and exposing its interior down to the knee. There was no abscess having defined walls; but an irregular cavity existed between the skin with its fat and fascia and the triceps muscle, which communicated with the outlet at the knee by one or two large sinuses. The muscles looked pale and shrunken, and as if their own investments had had most share in the profuse suppuration. In some respects the premonitory signs of death appeared to resemble so much those of pyæmia as to cause it to be suspected that this might be the cause of death. But this suspicion was not confirmed; for all the joints, including especially the left knee-joint, were quite healthy, and so were the lungs. The death was satisfactorily accounted for by the extensive suppuration amongst the muscles of the thigh, and by the irritative fever and exhaustion which followed in consequence.—*Lancet, Dec.* 17, 1859, *p.* 613.

54.—ON THE TREATMENT OF CONTRACTION OF THE KNEE.

By Holmes Coote, Esq., F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital.

[If cases the result of accident, or those proceeding from malignant disease, be omitted, undoubtedly the greater number of the remaining cases of anchylosis of the larger joints are owing to disease, either rheumatic or scrofulous in its nature. It is worthy of note that osseous anchylosis much more frequently follows rheumatic than scrofulous inflammation. The bones are of their normal weight and firmness in rheumatic, but in scrofulous diseases of joints these bones are light, and give every indication of want of power. In the museums, preparations of osseous anchylosis of the hip are much more common than those of a similar state of the knee.]

It is scarcely possible to attach too much importance to this fact respecting osseous anchylosis of the knee, for it serves us as a guide in most important points of treatment. In incipient cases of strumous disease, we may safely predicate that the contraction is chiefly due to muscular action; and we should, therefore, repudiate any such proceeding as that of "*the forcible extension of the limb, the patient being under the influence of chloroform*"; well knowing that, after the full action of the anæsthetic, the muscles relax of their own accord, and the limb becomes straight without the necessity of any force.

In some cases, there may be fibrous bands, which had, however, better be stretched than torn; or tendons may be so tight as to need subcutaneous division. The bones of the leg may be drawn backwards into the popliteal space, or the knee may be turned inwards, as in genu valgum; but there is still movement in the joint, except in cases where there has been an extraordinary amount of destructive disease; and it will be found that infinitely more good can be effected by steady, slow mechanical extension, than by any amount of sudden violence, however skilfully employed. But in cases where the joint has been destroyed by an attack of acute rheumatic inflammation, the opposed surfaces of the tibia and femur may become united by bone, the patella adhering to the outer condyle; and here mechanical extension often fails to act upon the limb. The resistance offered at the knee is such that the necessary counter-pressure bruises and excoriates the limb; and here we may with advantage forcibly break through the bony adhesions, which are rarely very strong; and then gradually extend by mechanical apparatus, as in the former case.

The question may be asked, Why not extend the limb at once, when the bony adhesions have been broken? The reply is, You wish to extend the limb without pain; without injury to the flexor muscles of the ham; without chance of stretching, tearing, or otherwise injuring the popliteal vessels and nerve. Some months ago, I amputated the limb of a young man, at his urgent request, in whom the knee-joint had long been contracted, and various methods of treatment had been adopted without success. He declined any further trials in this hospital, saying that he could not follow his avocation with a diseased knee. Extension had been tried on several occasions. On examining the limb after its removal, ecchymoses were found extending along the hamstring tendons; the muscular fibres had been torn and bruised; the popliteal vessels were so thickened and changed in structure that violent and sudden extension might have been followed by injury to their walls.

The patient needs the support of the muscles and tendons surrounding a joint; it is they which contribute so much to the strength of the limb. If they are torn and injured, the patient loses the necessary command over the bones.

And here it may not be out of place to state why it is that in cases of disease of the knee the leg becomes flexed on the thigh. When the leg is fully extended, the ligaments of the knee, especially the two lateral and the crucial ligaments, become extremely tight, and hold the bones firmly pressed against one another. The lower extremity moves in extension as one piece. Under circumstances of disease, this pressure cannot be borne; hence the muscles flex the leg, and, by so doing, relax the ligaments and set the bones free. This is another reason why forcible extension is unscientific and wrong in cases of progressive disease of the knee-joint. We should always wait until active disease has subsided.

It is not difficult to ascertain the nature of the contraction, whether muscular, fibrous, or osseous. In the first instance, the tendons of the hamstring muscles are at once felt quivering in spasm on the least attempt being made to extend the leg. In the second, the patient complains of pain about the ligamentum patellæ when the leg is drawn forward; the hamstring muscles also contract, though not to the same extent as in the former state. When the union is osseous, the parts are flexed and immoveable; extension seems to exert no influence on the muscles, and the patient complains but slightly of pain. There are, of course, other points which in each case will occupy attention; but these constitute the broad rule.

We may in very many cases promise to the patient, with safety, the advantage of an extended limb, which will be useful in progression; but we must not speak of "restored motion". When the tissues which are proper to a joint—namely, synovial membrane and articular cartilage, with the necessary ligaments—have been destroyed, free motion never can be regained. In order to effect such a phenomenon, we must supply our patient with a new joint. Let any one look around the different museums, and examine the specimens illustrating these diseases, and ask how free motion could have existed under the varied circumstances there presented for examination and reflection. But a moderate amount of movement may be very useful; and a stiff and extended limb is infinitely preferable to any artificial contrivance.—*British Med. Journal, Dec.* 17, 1859, *p.* 1015.

55.—ON SOME COMPLICATED INJURIES ABOUT THE SHOULDER-JOINT.

By FREDERICK C. SKEY, Esq., Surgeon to St. Bartholomew's Hospital.

[The following remarks are abstracts from a clinical lecture delivered at the Hospital by Mr. Skey. He commences with the most frequent form of injury to which the shoulder is liable, and proceeds subsequently to other injuries which, though not of infrequent occurrence, he thinks occasionally escape detection.]

If you inquire into the nature of the violence which has led to dislocation of the *humerus* (not of the *shoulder*, as it is commonly termed, for the shoulder, correctly speaking, is incapable of dislocation), you will generally make out that in the act of falling the man's arm has been extended for the purpose of breaking the fall; and that direct force has been applied on the under surface of the limb, whether hand fore or upper arm; the result of which is, that the head of the humerus is thrust out of the glenoid cavity, by a counter force rupturing the lower part of the fibrous capsule. The ultimate position of the head of the bone depends much on that of the arm at the moment of the fall; whether extended straight from the trunk,—the most common position, when the head falls into the axilla,—or forwards, or backwards, when it is found in either case in the opposite direction.

I shall, however, take as an example a case of dislocation into the axilla. The shoulder is flattened by the removal of the support, as it is termed, given by the head of the bone to the deltoid, which, being stretched, shows the clean outline of its origin. If you pinch it, you find it firm and tight—so tight, indeed, as to draw the elbow away to the extent of about four inches from the side of the body. If you attempt to approximate the arm to the trunk, the latter will recede, because the movement is a painful one, by drawing yet more positively on the already stretched deltoid and supra-spinatus muscles. If, on the contrary, the arm be raised, and the deltoid relaxed, pressure of the fingers upon it will detect the absence of the normal support it possesses in health. and the head of the bone will be felt in the axilla. If the hand be now applied in a similar manner to the opposite axilla, the different relative positions of the bones will be obvious; and, finally, it will be found on measurement that the affected arm is about an inch longer than the sound one. And this sign alone is conclusive as to the existence of dislocation, because the scapula being unchanged in its position, if the humerus extends an inch longer than the opposite bone, it is clear that its head cannot reach the socket. Add to the above the acknowledgment of a recent injury, and you have abundant evidence of simple dislocation of the humerus into the axilla.

The only variety of this accident to which I shall refer is that of dislocation forwards and inwards, in which the head of the bone is carried far upwards towards the clavicle, and when it is said to lie under the pectoralis minor or under the coracoid process, and which process obstructs its further progress in that direction. This is a rare variety of this dislocation, and I should not have alluded to it but for the purpose of marking two features peculiar to it: 1st, that the arm is rather shortened than lengthened; and 2nd, that it is usually accompanied by numbness of the arm, consequent on pressure of the head of the bone on the brachial plexus. The numbness accompanying ordinary dislocation is either absent or very partial; in this variety it is large and general. In both instances it subsides on the reduction of the displaced bone. I remember, some thirty-five years ago, a man being brought into the hospital with a dislocated humerus. While undergoing the necessary manipulation, distinct crepitus was felt in the joint. Its nature and locality were uncertain. The injury was very deliberately examined by the entire surgical staff. Much discussion arose on the case, which was evidently one of dislocation. However, the bone was reduced by the ordinary method adopted at that era,—namely, by extension of the arm outwards,—and the crepitus was felt no more. The man recovered in the usual time. A second case presented itself within the year, the entire features of which were precisely similar to the former one.

The impression produced on the minds of many observers of these cases has ever been, that it is possible to have, as the accompaniment of dislocation of the humerus, a crepitus, not caused by a fractured bone, but arising from some unusual injury sustained by the joint; and for many years I was myself inclined to adopt the same explanation, but later experience has taught me otherwise. It appears to me more reasonable to infer in these cases an undetected fracture, than the presence of crepitus without fractured bone. I cannot comprehend the sounds or sensations of crepitus to be caused by the attrition or rubbing together of any two structures of which the shoulder-joint is composed; and under these circumstances I cannot doubt that the injury in both these cases consisted of fracture of the glenoid cavity superadded to dislocation; and when it is recollected that the mode of reduction adopted at that period consisted in drawing the humerus outwards, it is not surprising that the fracture should escape detection during the operation; and if it then escape observation, it is not probable that it would attract notice afterwards, because the simple agency of rest would be available to the recovery from both the fracture and the dislocation.

While employing the term "crepitus," I may observe that there are three distinct varieties of sensation to which the term may be applied, and which it would be well for you to observe. The first is the sensation conveyed by the rubbing together of the two fractured ends of the shaft of a long bone in a healthy subject; the second, the sensation peculiar to the rotation of the thigh in fractured neck of the bone in very old or debilitated persons, which resembles the cracking of an egg-shell; the third, the sensation occasionally conveyed to the touch by the movements of a diseased joint. The last of these is very distinct from the other two, and is the product of tissues less hard than bone. The term "crepitation" is limited to the breaking down of membranous tissues by the pressure of air confined within them. In the two cases I have above quoted the crepitus was clear and distinct, and, as I consider, could have been obtained only by the attrition of a broken bone.

The form of injury which I am about to describe is that of fracture of the glenoid cavity. A man sustains a fall on the shoulder, with the arm parallel to the body, but the force of the blow, whether the result of a fall or of direct violence, is thrown *on the deltoid.* The result is the fracture of some bone, indicated by palpable crepitus. Observation will teach you to judge tolerably correctly, by the sensation thus obtained, whether the crepitus indicates a large or a small fracture, and such knowledge will prepare you for a correct diagnosis of the real injury. Now, the diagnosis of fracture of the glenoid cavity is obtained chiefly by negative evidence, and in recalling to your recollection the bony structures immediately around, you subject each to a careful examination. You examine the clavicle, acromion, and the spine and coracoid process of the scapula. You find all bear forcible pressure, and may therefore be inferred to be sound, you next examine the head of the bone. Pressing your hand firmly on the deltoid, you rotate the arm freely, and you find the head to move as freely with the shaft, and the movement to be unattended with pain. You repeat this examination by raising the arm at right angles with the body, and pressing the thumb forcibly on the head of the humerus in the axilla, you again rotate, and the movement is both painless and inaudible, revealing no fracture. Nothing remains but the scapula, which, if you are correct in your diagnosis of crepitus, must be broken. It can be nothing else. If you now, while pressing the head of the humerus against the glenoid cavity, rotate the bone, the mystery will be solved, and crepitus will be evidently the result of fracture of a greater or less portion of the glenoid cavity. This evidence can only be obtained in one way—viz., by pressure of the head of the bone against the broken socket. The existence of fracture is borne out by the direction as well as the violence of the force which has caused it, and by the presence of local pain, due not to the broken bone merely, but to a corresponding injury done to the soft parts over and around it. If you have arrived at your conclusions hesitatingly, if a doubt remain on your mind as to the existence of fracture, the patient himself will often solve it on putting the question to him—Have you broken a bone?—and especially if a man of somewhat more than ordinary intelligence; because, however palpable may be that compound sensation of hearing and touch we term crepitus, it is still more palpable to himself when his mind has been directed to it by the probably protracted discussion that has taken place on the nature of his accident. When a patient is made cognizant of the nature of the accident, his evidence becomes valuable. These cases cannot be readily mistaken for dislocation, for in truth they have no sign in common with it, except the disinclination to move the arm, which is universal in its application to all injuries in this region; but especially is there no diminution in the rotundity of the shoulder, and no increase in the length of the arm.

Several of these examples of fracture of the glenoid cavity have come under my observation during the last year. Both the nature and the extent of the injury are readily detectable by the inductive method of examination I have described. The extent of the fracture is not always so easily determined; yet by carefully observing the character and amount of the crepitus, and noting whether it is readily produced by the act of elevating the arm, and pushing the head of the bone against the socket, we shall make a close approximation to the truth.

Fracture of the glenoid cavity may be complicated with dislocation of the humerus; but, so far as my own observation leads me, I believe it to be far more uncommon than simple fracture without dislocation. If the fracture as usually occurs, when superadded to dislocation, is confined to a small portion of the glenoid cavity, the signs of fracture merge in the apparently larger accident of dislocation, the evidence of which is uninfluenced by the presence of the injury to the bone. The shoulder is flattened, the arm is lengthened, the head of the bone revolves on the thumb pressed upon it in gentle rotation of the bone, while the fractured scapula lies behind the head, and escapes observation, unless the violence has been unusually great and the fracture unusually large, and comprising one-half or more of the socket from which the head is thrown. The evidence of dislocation is conclusive, while that of fracture is so imperfect and is so uncommon, that the injury escapes observation. The heel is applied, the bone is reduced, and although the return of the head into the socket may be accompanied with some unusual sensation, which is really that of crepitus, the reduction has been so satisfactory, the restoration of parts so complete, that the question of fracture may scarcely enter the thoughts of the surgeon. Nor does there arise in the after-history of the case any circumstance by which to determine the occurrence of fracture. The fragments are brought into contact by the act of reduction, and in the course of a few weeks are united.

A case of more than usual interest occurred to me lately in the person of a man, aged sixty, in Abernethy ward, who had sustained an injury to the shoulder three weeks prior to his admission. The head of the humerus was obviously thrown from its socket, and the flattening of the shoulder was unusually great. The deltoid was tight, and the elbow was drawn from the side. The arm was lengthened by one inch. The head of the bone rotated with the shaft; the coracoid process was unbroken, but there was distinct crepitus on moving the arm. The man was very positive in his assurance that the injury was caused by a direct fall on the shoulder. I made an attempt to reduce the dislocation, under chloroform, by the aid of pulleys. I brought the bone towards its natural relation to the acromion process, by extension downwards, with a padded fulcrum in the axilla. The natural rotundity of the shoulder was restored during the extension, but on its remission it disappeared. On the second day following I repeated the attempt, more critically, persevering in the extension for nearly half an hour, but with no better success. While under extension the head of the bone became prominent, but returned to its abnormal position on remitting the extending force.

On reviewing all the circumstances of the case, there can be little doubt that the glenoid cavity is separated from the rest of the scapula either in a large proportion or absolutely. A healthy man, possessing the entire power of movement of his arm, sustains a fall directly on his shoulder; the head of the bone is struck inwards with force against the socket, and extreme flattening of the shoulder and crepitus are the immediate consequences; the arm is lengthened, and the elbow projects from the side; the unbroken condition of the head is rendered obvious by its free rotation under pressure; the coracoid process is firm in its position; while two attempts at reduction fail to restore the head of the bone to the glenoid cavity. In what can this injury consist, if not in extensive fracture of the glenoid cavity—the socket of the joint? I believe the fracture to be a large one, because, having seen many cases of fracture of the lower portion of the glenoid cavity, I have never observed one example in which the symptoms were so strongly marked as in this case; while the slipping back of the head of the bone, after being brought on two occasions to its normal position, appears to infer the absence, and therefore the inability, of the socket to retain it. The only treatment the case appeared to me to be susceptible of, was that of placing a large pad in the axilla, and leaving the rest to nature; and this plan was adopted.

In cases of fracture of a greater or less portion of the glenoid cavity, an easy position of the arm is all that is attainable, and indeed all that is required. The fragments are usually united in three weeks, during which the arm is placed in a sling.

I had recently another case of fractured scapula under my care in St. Bartholomew's Hospital. This fracture was the result of a fall with violence, but the precise direction of the blow I could not ascertain. The fracture extended vertically through the bone, either through or behind the notch. It was unattended by dislocation, or even by displacement. Its existence was ascertained by the presence of crepitus, which was well marked, and might be detected either by the pressure of the fingers, or by desiring the patient to put into action the trapezius muscle of that side. A large pad was placed upon the scapula, which was firmly bandaged to the trunk. His recovery was both quick and complete. I have described elsewhere a case of fracture of the inferior angle of the scapula. The injury occurred to a lady, who was thrown from her horse. In this case I could only obtain crepitus by bringing into action the teres major muscle, which takes its origin from the surface of the scapula, through which the fracture had occurred.—*Lancet, April* 14, 1860, *p.* 368.

56.—*On Removal of Articular Cartilage in Amputation.*—[At a meeting of the Medical Society of London, Mr. Haynes Walton stated that he believed the usual impression among surgeons, as to the propriety and necessity of removing the articular cartilages when amputating at a joint, was fallacious.]

He had lately watched two amputations of the knee, and several about the large joints of the foot and arm, all of which had united by the first intention. He had lately removed a hand, shattered by the bursting of a gun, at the carpo-metacarpal articulation, leaving the carpus entire, bringing the flaps together over the irregular edge formed by the second row of the carpal bones covered by their cartilage, and a more rapid union he had never seen. The patient could move the carpus, and the radio-ulnar action was natural—no small advantages for the adaptation of an artificial hand. He thought the great advantage of not interfering with the cartilage was the less likelihood of necrosis of the end of the bone, or of suppuration, and other evils often attendant on cutting through a bone, or exposing its extremity.—*Lancet, Nov.* 5, 1859, *p.* 469.

57.—SCOOPING OF BONE SUBSTITUTED FOR RESECTION OR AMPUTATION.

M. SEDILLOT, of Strasburg, has, for the last two years, seized every opportunity, in cases of diseased bone, of scooping out the affected parts, and leaving the cortical portions, rather than have recourse to resection or amputation. In April 1858, he brought this mode of operating before the Academy of Sciences at Paris, and then stated that he was led to adopt the method from observing the remarkable osteogenic powers of the periosteum pointed out by M. Flourens and M. Ollier. Instead, however. of dissecting the periosteum from the bone, and removing the latter, (a proceeding which offers some difficulty), M. Sédillot leaves the whole cortical portion of the bone, as above stated, and removes the carious parts.

On the 31st of October last, the author brought before the same Academy an account of the cases operated upon in that manner. Ten patients recovered, and three died. Amongst the former, M. Sédillot mentions the case of a young girl, in whom he had scooped out the lower third of the femur and the condyles: she now walks very well. Another case is that of a young man suffering from caries of the lower part of the left tibia; the scooping here included the whole of the articular extremity, and the inside of the malleolus: the patient now works hard, and can walk fifteen or sixteen miles. The fatal cases may not be charged to the operation: one died of epidemic sloughing phagedena six weeks after the scooping, and the others several months after submitting to operative procedures. M. Marmy, of Lyons, and M. Ehrmann, a military surgeon in Algeria, have both sent to the author a successful case of this operation.

If we are not much mistaken, M. Sédillot's operation has much analogy to the gouging in caries so often successfully practised in the hospitals of this metropolis. But a real improvement in the operation of resection of joints is, the careful preservation of as much periosteum from the extremity of the articular surfaces some distance up the shafts as has not been destroyed by the progress of disease. This should be borne in mind by those surgeons who frequently perform resection of joints; nor are the practical proofs of the utility of these precautions wanting. M. Verneuil, of Paris, for instance, has placed several cases of resection of the elbow before the Academy of Sciences, and shows that, by dissecting very carefully whatever periosteum is left, he had, by regeneration of bone, in one of his cases, only two inches' shortening, after having removed altogether four inches of osseous texture from the humerus, radius, and ulna. In another case, M. Verneuil was able to leave a regular cylinder of periosteum at the lower extremity of the shaft of the humerus; and in this instance, also, the results were extremely satisfactory.—*Lancet, Dec.* 10, 1859, *p.* 591.

58.—*Forcible Flexure in Anchylosis.*—Since the attention of the profession has been drawn to the subject of the treatment of false anchylosis by forcible rupture of the uniting medium, through the labours of Mr. Brodhurst in this interesting field of research, we have noticed very many attempts to treat such cases at the different hospitals, and mostly with success, when the union has not been osseous. Several examples of the kind have been recorded in our 'Mirror.' Two lately occurred at St. George's Hospital, in which this plan of treatment has been followed by the most satisfactory results. The first was a boy who had broken off the outer condyle of his right arm four months ago. This united, with the arm remaining in a faulty position from a false anchylosis; pronation and supination were perfect, but a stiff joint remained. Under chloroform, Mr. Cæsar Hawkins readily broke up the uniting medium, and the fullest flexion and extension were obtained, and this has continued without an untoward symptom. The other case was that of a woman aged fifty-five years, who fell on her hand three months and a half ago, fractured the lower part of the radius, and injured the elbow, partially dislocating the ulna backwards. The result was union of the fracture and anchylosis of the elbow-joint, without the slightest motion, the arm being, moreover, almost straight, and to all intents useless. She was seen by Mr. Prescott Hewett three weeks before admission, and it seemed a case suitable for forcible flexion. She was given chloroform, on the 12th of November, and on firmly laying hold of the forearm, Mr. Hewett readily broke up the adhesions, and flexed the arm with the greatest ease, restoring the normal position, without being accompanied by the slightest rigidity. This patient is now doing remarkably well.

The success which has attended this plan of treatment in various other joints besides the elbow, in the hands of Mr. Brodhurst and numerous other surgeons, entitles it to every consideration.—*Lancet, Nov.* 26, 1859, *p.* 536.

59.—*Manufactured Pasteboard Splints.*—[At a meeting of the Royal Medico-Chirurgical Society, Mr. Acton exhibited some pasteboard splints, such as were used extensively by the Russians at Sebastopol. Their inventor is M. Merchie, chief surgeon to the Military Hospital at Brussels.]

They are exceedingly light, firm, and inexpensive, being manufactured in pasteboard or *papier maché.* When wet, the substance is moulded to the form of the thigh, leg, arm, or forearm. These portable dry splints retain the proper shape of the limb. In case of fracture occurring, half a splint is applied to the limb, a little cotton wool only intervening between the skin and the pasteboard. The other half-splint is then placed on the opposite side of the fractured limb, and the whole enveloped in a bandage. The inventor had demonstrated, on the field of battle, that, as soon as these splints were applied, a man could be removed to the rear, and transported long distances, without danger or pain, in the usual conveyances.

[These splints must not be confounded with splints made by moulding cardboard to the injured limb; they are made by moulding pulped paper to shapes suitable to every size of limb, then dried, and kept ready made for use.]—*Lancet, Dec.* 3, 1859, *p.* 564.

ORGANS OF CIRCULATION.

60.—ON ACUPRESSURE—A NEW METHOD OF ARRESTING SURGICAL HEMORRHAGE.

By J. Y. Simpson, M.D., Professor of Midwifery in the University of Edinburgh, &c.

[At the first winter meeting of the Royal Society of Edinburgh, in December, 1859, Professor Simpson made a lengthened communication on acupressure, a new mode of arresting surgical hemorrhage. After passing in review the various means employed in arresting surgical hemorrhage, and observing that ligatures, however employed (even metallic) always act as foreign bodies, and excite higher stages of inflammation than the adhesive, Dr. Simpson said, that what is wanted in plastic surgical operations in order to ensure union by the first intention, is, to arrest the hemorrhage effectually, and yet not leave any foreign body whatever in the wound. This end is attained by acupressure.]

Dr. Simpson stated that he had tested the effects of acupressure as a means of effectually closing arteries and stanching hemorrhage first upon the lower animals, and lately in two or three operations on the human subject. The instruments which he proposed should be used for the purpose, were slender needles or pins of passive iron, headed with wax or glass, and in other respects also like the hare-lip needles commonly used by surgeons at the present day, but longer when circumstances require it. They might be coated with silver or zinc on the surface, if such protection were deemed requisite.

At first, Dr. Simpson believed that in using acupressure as a hemostatic means, it would be necessary to compress the tube of the bleeding artery between two needles, one placed on either side of it. But in his later experiments upon the living as well as the dead body (as in amputations on the latter, and subsequently injecting tepid water through the arteries, in imitation of the flow of blood), he had found, that the compression of one needle was usually perfectly sufficient to shut up an artery, and that even sometimes, when two or more bleeding points were near, they could be closed simultaneously by the action of one needle or pin. The whole process consists in passing the needle *twice* through the substance of the wound, so as to compress together and close, by the middle portion of the needle, the tube of the bleeding artery a line or two, or more, on the cardiac side of the bleeding point. The only part of the needle necessarily left exposed on the fresh surface of the wound is the small middle portion of it, which passes over and compresses the arterial tube; and the whole needle is withdrawn on the second or third day, or as soon as the artery is supposed to be adequately closed, thus leaving *nothing* whatever in the shape of a foreign body within the wound, or in the tissues composing its sides or flaps. To produce adequate closing pressure upon any arterial tube which it is desired to constrict, the needle must be passed over it so as to compress the tube with sufficient power and force against some resisting body. Such a resisting body will be most frequently found, 1st. in the cutaneous walls and component tissues of the wound; 2nd, sometimes in a neighbouring bone, against which the artery may be pinned and compressed by the acupressure needle; and, 3rd, in a few rare cases it may possibly be found in practice, that a second needle may require to be introduced to serve as a point against which the required compression is to be made. Most commonly the first of these three plans seems perfectly sufficient, and that even in amputation of the thigh. In acting upon this mode, the surgeon may place the tip of the fore-finger of his left hand upon the bleeding mouth of the artery which he intends to compress and close; holding the needle in his right hand, he passes it through the *cutaneous* surface of the flap, and pushes it inwards till its point project out to the extent of a few lines, on the raw surface of the wound, a little to the right of, and anterior to his finger-tip; he then, by the actions of his right hand upon the head of the needle, turns and directs the needle, so that it makes a bridge as it were *across* the site of the tube of the bleeding artery immediately in front of the point of the finger, with which he is shutting up its orifice; he next, either with this same fore-finger of the left hand, or with the side of the end of the needle itself, compresses the locality of the bleeding arterial orifice and tube, and then pushes on the needle with his right hand so as to make it *re-enter* the surface of the wound a little to the left side of the artery; and lastly, by pressing the needle farther on in this direction, its point re-emerges through the *cutaneous* surface of the flap,—and the site of the tube of the bleeding artery is in this way left pinned down in a compressed state by the arc or bridge of steel that is passed over it. The needle thus passes first from and through the skin of the flap *inwards* to the raw surface of the wound, and after bridging over the site of the artery, it passes secondly from the raw surface of the wound *outwards* again to and through the skin. Sometimes the needle will be best passed by the aid of the eye alone, and without guiding its course by the finger-tip applied to the bleeding orifice. It compresses not the arterial tube alone, but the structures also placed over and around the *site* of the tube. When the needle is completely adjusted, all of it that is seen on the surface of the raw wound, and that not necessarily so, is the small portion of it passing over the site of the artery, while externally, upon the cutaneous surface of the flap, we have remaining exposed more or less of its two extremities, namely, its point and its head. The rest of it is hidden in the structures of the flap or side of the wound. The degree of pressure required to close effectually the tube of an artery is certainly much less than medical practioners generally imagine; but in the above proceeding the amount of pressure can be regulated and increased, when required, by the acuteness of the angle at which the needle is introduced and again passed out,—the cutaneous and other structures of the flap serving as the resisting medium against which the needle compresses the arterial tube. But if it were ever, perchance, necessary to produce greater compression than can be thus accomplished by the needle alone, this increased pressure could be readily obtained by throwing around the two extremities of the needle exposed cutaneously a figure-of-eight ligature, as in hare-lip, with or without a small compress placed between the arc of the ligature and the skin. The process of the adjustment of the needle is difficult to describe shortly by words, but the whole of it is readily seen and imitated when repeated upon a piece of cloth or leather. We fasten the stalk of a flower in the lapelle of our coat by a pin passed exactly in this manner. To compress a bleeding artery against a bone is somewhat more complicated, but not much so. In accomplishing it, we have to introduce from the cutaneous surface a long needle through the flap of the wound obliquely to near the site of the artery, and then compressing, with the fingers of the other hand, or with the end of the needle, the part containing the artery against the bone, we make the needle, after passing over this compressed part, and after testing whether it has closed the vessel or not, enter into the tissues beyond, and if necessary even emerge from, the cutaneous surface on the other side at an angle somewhat oblique to that at which it entered; thus taking advantage of the resiliency and resistance of the soft textures to make them push the needle with the necessary degree of compression against the artery and bone. Arteries in particular parts require special adjustments and modifications to compress them against the neighbouring bone, which only experience can point out. There is always sufficient soft tissue on either side of the artery for the needle to get a purchase upon, to compress the arterial tube against the bone or other resistant point. In two cases, Dr. S. had found that branch of the internal mammary artery which so frequently bleeds in the bottom of the wound after excision of the mamma, easily and perfectly closed by a needle passed through the flap to near the artery, then lifted over it and (after compressing it so as to stop the flow of blood) pushed onwards into the tissues beyond. Possibly, in some amputations, an acupressure needle or needles may yet be passed, immediately before the operation, half an inch or so above the proposed site of the amputation line, so as to shut the principal artery or arteries, and render the operation comparatively bloodless. If so, these needles would serve, at one and the same time, the present uses of both tourniquet and arterial ligatures. Perhaps this will be found, in some cases, a simple and effectual means of compressing and closing the artery leading to an aneurism,—as the femoral artery, for example, in popliteal aneurism,—changing the operation for that disease into a simple process of acupuncture instead of a process of delicate dissection and deligation, when in any case the milder methods of compression, manipulation, and continuous flexion of the limb fail. It has been hitherto a difficult problem to obstruct the vessels of the ovarian ligament in ovariotomy, without leaving a foreign body, whether clamp or ligature, upon the stalk of the tumour, to ulcerate and slough through it. If the stalk be transfixed and pinned in its whole breadth to the interior of the relaxed abdominal walls, by one or more acupressure needles passed through these abdominal walls from without, this difficulty may possibly be overcome.

That needles used for the purpose of acupressure, and passed freely through the walls and flaps of wounds will not be attended by any great degree of disturbance or irritation, is rendered in the highest degree probable by all that we know of the tolerance of living animal tissues to the contact of metallic bodies. Long ago, John Hunter pointed out that small-shot, needles, pins, &c., when passed into and imbedded in the living body, seldom or never produced any inflammatory action, or none at least beyond the stage of adhesive inflammation, even when lodged for years. Some time ago, when the subject of acupuncture specially attracted the attention of medical men, Cloquet, Pelletan, Pouillet, and others, showed that the passage and retention of long acupuncture needles was attended with little or no irritation in the implicated living tissues. The Reviewer of their works and experiments in the 'Edinburgh Medical Journal' for 1827, observes, —"It is a remarkable circumstance that the acupuncture needles never cause inflammation in their neighbourhood. If they are rudely handled or ruffled by the clothes of the patient, they may produce a little irritation; but if they are properly secured and protected, they may be left in the body for an *indefinite* length of time without causing any of the effects which usually arise on account of the presence of foreign bodies. In one of M. Cloquet's patients, they were left in the temples for eighteen days; and in cases in which needles have been swallowed, they have remained without causing inflammation for a much longer period. It appears probable, from the facts collected on the subject, that metallic bodies of every kind may remain imbedded in the animal tissues without being productive of injury." All the late observations and experiments upon metallic sutures are confirmatory of the same great pathological law of the tolerance of living tissues for the contact of metallic bodies imbedded within their substance. In the operation for hare-lip, where the whole success or failure of the operation depends on the establishment or not of union by the first intention, surgeons use needles to keep the lips of the wound approximated, often compressing these needles strongly with their figure-of-eight ligatures, and find this measure the most successful means which they can adopt for accomplishing primary adhesion.

The acupressure of arteries, when compared with the ligature of them, appears, as a means of arresting hemorrhage, to present various important advantages :—1st. It will be found more easy, simple, and expeditious in its application than the ligature. 2nd. The needles in acupressure can scarcely be considered as foreign bodies in the wound, and may always be entirely removed in two or three days, or as soon as the artery is considered closed ; whilst the ligatures are true foreign bodies, and cannot be removed till they have ulcerated through the tied vessels. 3rd. The ligature inevitably produces ulceration, suppuration, and gangrene at each arterial point at which it is applied ; whilst the closure of arterial tubes by acupressure is not attended by any such severe consequences. 4th. The chances, therefore, of the union of wounds by the first intention should be greater under the arrestment of surgical hemorrhage by acupressure than the ligature, 5th. Pyæmia and surgical fever seem not unfrequently to be excited by the unhealthy suppuration, &c., in wounds which are liable to be set up by the presence and irritation of the ligatures. 6th. These dangerous and fatal complications are less likely to be excited by the employment of acupressure, seeing the presence of a metallic needle has not the tendency to create local suppurations and sloughs in the wound, such as occur at the seats of arterial ligatures. And 7th, Hence, under the use of acupressure, we are entitled to expect both, *first*, that surgical wounds will heal more kindly and close more speedily; and *secondly*, that surgical operations and injuries will be less frequently attended than at present by surgical fever and pyæmia. —*Edinburgh Med. Journal, Jan.*, 1860, *p.* 647.

[In a subsequent communication in the 'Medical Times and Gazette,' on the subject of Acupressure, Dr. Simpson continues :—]

The great practical question, however, is—granting that acupressure by metallic needles is simpler and safer than the silken ligature—Is it actually sufficient for the closure of the arteries usually divided in surgical operations? Before publishing on the matter I had experimented on the subject in the lower animals, and had shut up easily by it the carotid artery of the horse—the largest living vessel which it was within my power to experiment upon. I had made various experiments with it, in the way of amputation and other experiments, on the dead human subject—imitating the flow of blood by the injection of tepid water along the arteries; and, lastly, I had used it with perfect success in the living human subject, in shutting up the arteries that were divided in three cases of excision of the cancerous mamma. In the last of these three cases I had to close six separate arteries with an equal number of acupressure needles. But, not being a practical surgeon, I had, of course, no opportunity of testing it myself in any of the graver operations of surgery, such as amputations of the limbs. Through the kindness, however, of my surgical friends, I am able to report the effects of acupressure in four cases of amputation in which it has been employed during the past month.

For the first application of acupressure to the arrestment of hemorrhage after amputation of the limbs, I am indebted to one of the most accomplished and advanced surgeons in this country—my friend Dr. Greig, of Dundee. Formerly as surgeon to our army in the East, and latterly as surgeon to the large hospital at Dundee, Dr. Greig has enjoyed great opportunities as an operator. His very interesting letters to me on the subject of acupressure indicate the change and struggle which every earnest and ingenuous mind has in setting aside old-established and cherished practices for the adoption of what is new. When I first took the liberty of directing his attention to the subject, as contained in an abstract of a paper on it in the January number of the 'Edinburgh Medical Journal,' and asked him to be so good as to test the plan, he wrote me, January 8, 1860, that he could "see no great difficulty in giving the thing a fair trial." But he adds:—"Of its general adoption I have great doubts. We have been always taught to look upon the ligature as the only true means of arresting hemorrhage, and this feeling is somewhat difficult to get out of one's mind. Your illustration of fixing a flower in the lapel of the coat by means of a pin, explains the whole thing."

Case 1.—Two days afterwards, January 10, Dr. Greig wrote me:—"I performed amputation at the forearm this afternoon in a case of laceration of the hand from the bursting of a gun, and I used the needles instead of ligatures for arresting hemorrhage. Both the radial and ulnar arteries bled freely, but were easily controlled by a needle placed on each, almost half-an-inch above the cut end. Both needles were, of course, in the palmar or anterior flap, and were applied *quite as easily as a ligature*." These last words are underscored in Dr. Greig's letter, and show that thus the very first trial of acupressure proved as easy as deligation in the hands of a surgeon who for years had been in the constant practice and habit of applying ligature to arteries for the staunching of hemorrhage in his operations.

Case 2.—Three days afterwards, January 13, Dr. Greig again wrote as follows:—"I have had another amputation at the middle of the forearm to-day, and used acupressure with ease and success. The process, so far as I have tried it, is *the simplest* one can imagine; and, unless I see some good reason for changing my mind, it must ultimately come into universal adoption. It is really surprising how very little pressure is required to stop bleeding from an artery. In fact, I had no idea of it till I tried acupressure."

On January 20, Dr. Greig writes:—"Both the cases of amputation in which I used acupressure have done remarkably well. There has been less irritation and less suppuration, and the wounds are healing more kindly than had ligatures been employed. The first case did not close by the first intention, owing to part of the anterior flap having been lacerated by the explosion. The second has gone on as well as could be wished—no fever, no irritation—and the wound is healing by the first intention. What surprises me more than anything else is, the very small amount of pressure which is required to stop arterial hemorrhage. In passing the needle over an artery I do not think it will be found necessary to turn it sharply over the vessel, thereby binding it very tightly to the flap. Such a degree of pressure is by no means required. Less irritation is caused by passing the needle more lightly across the artery, and taking in more tissue along with it."

"It is a great comfort also," Dr. Greig adds, "to both patient and surgeon, that by acupressure the artery is closed in about forty-eight hours (a large artery may, of course, require a longer time), and all cause of irritation at once removed. In my first case I allowed the needles to remain in for three days; but in future I will consider two days long enough; and, for all I know, perhaps it is longer than is required."

"I have now the greatest faith in acupressure. I intend employing it in all kinds of cases that may come under my care, and I will have no fear whatever to use it in my first thigh amputation."

"In giving directions for securing the vessel, you advise the surgeon to place the forefinger over its bleeding mouth, &c. Now you will find it much better when you have a flap to keep the finger of the left hand on the skin side and use the thumb. You feel the vessel beating between the thumb and forefinger, and you can introduce the needle in the dark."

I heard again from Dr. Greig on January 23. "The amputations," he states, "are doing well, and both patients are walking about the wards." "Yesterday," he continues, "at a case of removal of the mamma, I again used the needles, and easily arrested the hemorrhage from two arterial branches which were spouting freely in the upper or axillary flap. A small branch of an intercostal was the only other bleeding vessel, and torsion was used for it. Nothing could have been easier or more beautiful than acupressure applied in this case, as the procedure was seen in its simplest form—more so than in a flap." "I see," Dr. Greig adds, "that in France M. Foucher has tried acupressure on the dead subject, and also on a dog. I wonder why he did not try it in an amputation. Nothing can be easier, and if a surgeon uses it once I am sure he will do so again.—*Edin. Med. Journal, Jan.* 1860, *p.* 647, *and Med. Times and Gazette, Feb.* 11, 1860, *p.* 137.

61.—ON ACUPRESSURE.

[A writer in the 'Med. Times and Gazette,' signing himself A. M., observes that in none of the cases related by Dr. Simpson, in support of the use of acupressure in amputations, did complete reunion by the first intention take place. In other words, the object of these appliances has not been fully attained.]

Some of the causes of this failure seem to me clear.

There is, first, the application of towels, lint, sponges, &c., to the raw surfaces, by which they are covered with myriads of small filaments or foreign bodies, each acting as a centre of irritation, and demanding ejection from the wound, and often, in the aggregate, constituting a more formidable evil than two or three silk or cotton ligatures. The experience of every housemaid in cleaning and drying crystal will illustrate and prove what I say; but direct proof can be found in the microscopic examination of the early discharges from a wound so handled.

The saw is a further source of dead or foreign bodies, the flaps being frequently, if not generally, dusted over with powdered bone.

The second cause of failure, I think, arises from the application of warm or cold water to the bleeding surfaces. In this I may be in error, though I believe I have had proof of the soundness of the conclusion. Water thus applied causes the death, if I may so speak, of a certain amount of blood, which then lies in the wound as a thing to be removed; or, stating it differently, the use of the water induces such changes in the blood oozing from the cut surfaces, as renders it unfit to undergo those changes through which it should pass in the process of healing or union, or, in other words, unfit to serve those purposes which it is intended to serve.

The employment of the needles in arresting the hemorrhage makes the use of towels, sponges, and water, all but unnecessary, as the bleeding mouths of vessels have not to be sought for, as is the case when ligatures are used.—*Med. Times and Gazette, March* 3, 1860, *p.* 225.

62.—*On Acupressure.*—Acupressure is fast coming into use among surgeons. In Mr. ADAMS' case, which we noticed last week, the superiority of the needle over the ligature has been made most apparent. The needle was removed forty-eight hours after the operation, and there was not a sign of bleeding or suppuration; while the ligature has remained, as usual, acting as a seton during the sloughing process it sets up. It should be observed that no part of the needle was left exposed on the raw surface of the wound; it was introduced half-an-inch above the cut point. This is a great advantage in amputation; for the cut surfaces can be brought into apposition free at every point from the contact of any foreign body. Our provincial hospital surgeons are taking up the method in a most creditable manner. At Dundee, Carlisle, Greenock, and Liverpool, it has been applied with great success ; and now that Mr. Adams has led the way in the metropolis, at a small hospital, his example will be followed sooner or later in the larger establishments where the surgery partakes more of the character of stereotype. Mr. Bickersteth, of Liverpool, writes most warmly as to the facility of acupressure and its probable great results.—*Med. Times and Gazette, Feb.* 25, 1860, *p.* 194.

63.—*On Acupressure.* By HENRY T. TAYLOR, Esq., Guildford.—Having observed with much interest the success of Dr. Simpson's plan of acupressure in securing the vessels in amputations, I am led to think that the same principle might be applied to the deligation of arteries for aneurism or secondary hemorrhage, some modification being made in the form of the compressor, so that it should act exclusively on the vessel, and admit of easy withdrawal. The following simple means have occurred to me as likely to effect this purpose.

Having laid bare the carotid of a sheep, I passed under it a common bodkin of the kind used by sempstresses ; into the long eye of this I inserted a hare-lip pin, having first removed the point, and bending it down over the artery, I tied the ends of the pin and bodkin firmly together with a few turns of waxed thread. The pressure thus made arrested all pulsation in the vessel above the point included. After forty-eight hours I removed the compressor easily, cutting the thread and drawing the pin out first. On the next day, when the animal was killed, I found the artery firmly plugged with a coagulum of the usual form.

This operation will remind many of your readers of old attempts to obtain closure of the arterial tube by means of Assalini's compressor and the *presse-artère* of Dessault and Deschamps ; but it is less open to the objections urged against the use of those instruments, as it can be performed with less disturbance of the vessel, while the presence of so small a foreign body excites so much less irritation in the wound. It is worthy of consideration whether these plans were not too hastily abandoned, which possessed this advantage over the ligature—security from the dangers of secondary hemorrhage, where the coats of the vessel are unsound, or where the proximity of a collateral branch prevents the formation of a clot on the distal side of the ligature.—*Med. Times and Gazette, March* 24, 1860, *p.* 301.

64.—*Hemorrhage from the Tonsils after Incision.—Arrest by the Perchloride of Iron.* (Case under the care of Mr. HENRY THOMPSON, Surgeon to the Marylebone Infirmary.)—[A charwoman, aged 35, was admitted into the Marylebone Infirmary, suffering from acute tonsillitis. Two days previously a surgeon had punctured one tonsil in the hope of letting out pus, and on the following day had punctured the other tonsil; but in neither case was any pus evacuated, though there was free bleeding from both, which, however, soon ceased. On the evening of her admission, however, hemorrhage commenced from the right side, and continued without intermission for nearly two hours, by which time she had altogether lost about four pints, and the patient was pale and almost pulseless. The small opening in the right gland could be felt distinctly with the finger.]

The woman's condition being such as to admit of no delay, Mr. Thompson at once prepared to place a ligature on the carotid, and the needful instruments were got in readiness. Before doing so, however, he determined, though without much hope of being successful, to make a trial of the perchloride of iron. This was done by carefully mopping the part dry by means of lint wrapped on the left forefinger, and then pushing a glass-brush dipped in the fluid into the incision. After a few applications the bleeding was entirely stopped; in about a quarter of an hour, however, a slight recurrence necessitated a repetition of them, which were again successful. No return took place afterwards. The woman slowly regained her strength, and made ultimately a complete recovery.

Mr. Thompson stated that, although he had on several previous occasions easily arrested very formidable hemorrhages by resort to the perchloride solution, yet that on the present occasion he had not at all expected to be able to do so. He spoke most strongly as to the value of this agent when properly prepared according to the French codex.

[Of course, from the success of the treatment, it is all but certain that the carotid could not have been wounded.]—*Med. Times and Gazette, Dec.* 24, 1860, *p.* 631.

65.—*The Effects of the Perchloride of Iron.*—The influence of this powerful styptic was well shown in the arrest of dangerous hemorrhage from the tonsils, in a case which we noticed in our 'Mirror' of the 19th November last, under Mr. Henry Thompson's care at the Marylebone Infirmary. We have seen it used at University College Hospital for the coagulation of the blood in varicose veins, but in one instance it produced suppuration over the vein and much constitutional disturbance. It has been employed by Mr. Prescott Hewett, at St. George's Hospital, upon a mass of veins situated on the inner ankle of a young woman, with partial benefit; but the veins were large, and difficult to reduce either in number or bulk. In the same hospital we saw, on the 22nd ult., an infant who had a large congenital nævus of the left cheek. This had been treated by the injection of the perchloride of iron by a general practitioner out of the hospital, but with the effect of producing the most intense inflammation, and sloughing of the central portion of the nævus right down to the mucous membrane. Mr. Hewett has completely obliterated all the vessels of this nævus by a series of operations, which consisted in the passage of needles through its various parts, for the purpose of pressure and strangulation. The cure has been a tedious one, but nevertheless satisfactory.

The perchloride of iron would seem to be beneficial in some cases when it is introduced subcutaneously, but in others it sets up some amount of inflammation, which may so impair the vitality of the parts as to produce actual destruction of tissue, as occurred in the last-mentioned instance. It requires to be used with care.—*Lancet, Jan.* 21, 1860, *p.* 63.

66.—*Collodion in Arrest of Hemorrhage from Leech-bites.*—M. Stanislas Martin observes that a few layers of collodion promptly arrests the bleeding from leech-bites.—*Med. Times and Gazette, April* 14, 1860, *p.* 377.

67.—ON THE TREATMENT OF VARICOSE VEINS OF THE LEGS, AND OF VARICOCELE.

By John Erichsen, Esq., Professor of Surgery and of Clinical Surgery in University College, &c.

[The danger of operating in varix is slight, if the veins are not opened. If the veins are opened, air is admitted into the wound, and the chances of suppuration, instead of adhesive inflammation are much increased.]

There are many modes of treatment in varix, all having one object in view—viz., that of causing occlusion of the vein by the adhesive inflammation. But, in my opinion, all measures should be avoided which include opening the vein, whether this be done by caustic or by the knife, as they are extremely dangerous, by leading to suppuration within; so, also, are those plans of treatment by which we break down the exuded lymph and coagula, or open up the vein even when occluded.

I shall not enumerate all the methods before the profession for the radical cure of varix, but proceed to describe those which I have for many years successfully employed. The plan I ordinarily pursue is as follows:—A hare-lip pin is passed underneath the vein on one side, and its point brought out on the other, a piece of elastic bougie, about an inch in length, is then laid over the vein parallel to its course. Then, by means of a silken thread twisted over the bougie and under the two ends of the pin, the vein is compressed between the pin and bougie. In performing this operation, there are one or two little points to be observed. The first is to be careful not to transfix the vein with the pin; if the vein be opened, and the pin lie across it, there is danger of suppuration, as the pin acts as a kind of seton in the vein. This inadvertence is avoided by dipping the pin deeply, whilst passing it under the vein. In this way, there is no risk of piercing the vessel. If a drop or two of venous blood exude, by the side of the pin, through the puncture, the vein has been perforated, and the instrument should be withdrawn, and passed again. If the vein is unharmed, the operation is bloodless. The second point is, that the ligature should not be so tight as to cause ulceration by strangulation of the parts compressed. If the thread be moderately tight only, and the pin made, as it generally now is, of unoxidisable iron, it is quite passive, and does not rust, hence excessive irritation is avoided. The bougie and pin should remain about ten days, at the end of which time the vein is converted into an impervious cord of plastic matter and coagulum. When this is attained, the pin may be removed, the limb bandaged, and the patient may leave his bed.

It has been objected to this and to all other operations for varix, that the cure is not permanent; that the varicose condition is apt to return; that the same veins perhaps are not affected again in this manner, but that others speedily assume a dilated and tortuous state. No doubt this is the result in some instances, but in many cases which I have had an opportunity of examining years afterwards, the cure has been permanent; and, in the meantime, the object for which the operation was undertaken is served.

Another method which is much used in France, and which I have occasionally employed myself in this hospital for some years past, is that of injecting a small quantity of a solution of the perchloride of iron, of a specific strength, into the veins, by means of Pravaz's screw-syringe. In this way the blood contained in the dilated vessel is made to coagulate, and thus the passage through the vessel is occluded. The adhesive inflammation at the same time being excited, permanent obstruction is attained, and a cure effected. This, though a valuable means when the vessels are knotted and sacculated, is not, I believe, so good a one as the pin and ligature, because I have seen it followed, in two or three of the cases in which I have had occasion to use it, by circumscribed abscesses, and even sloughing of the adjacent parts, though no fatal result has, as yet, occurred in my practice. On this account, I consider this mode of treatment undoubtedly more dangerous; and I think it ought to be confined to the cure of those cases only where the knots are so large, and so closely matted together, that the pin cannot be passed underneath them.

The next distribution of veins liable to varix is that formed by the spermatic plexus. Dilatation of these vessels—varicocele—is often met with in young men; and much benefit can be afforded by palliative means, such as supporting or compressing the tumour in various ways; for instance, by raising the scrotum in a suspensory bandage, or by wearing a mocmain truss, &c., and one or other of these contrivances generally gives sufficient relief for the patient's comfort. But I have found it necessary to have recourse to more active measures than these, and to adopt operative treatment in three of the cases which have presented themselves during the present session. The circumstances for which operation may be and has been practised, in these and similar cases, can be arranged in the four following categories:—

1. The existence of a varicocele disqualifies the sufferer from admission into the public services. This, in my opinion, is a perfectly legitimate reason for operating. One of the cases on whom I recently effected a radical cure, was that of a man in the prime of life, who, wishing to enlist in the Marines, was refused solely on the ground of having a small varicocele. This I cured by operation, and the man afterwards entered the service.

2. In cases in which the presence of a varicocele of inordinate size causes a distressing sense of weight and pain in the loins and groins, and often inability to stand or walk for any length of time, in these cases, when the patient is in continual discomfort, or more or less prevented from pursuing his ordinary avocations, in fact, quite crippled, it is perfectly justifiable to resort to operation.

3. When atrophy of the testicle is a consequence of the pressure of the blood in the veins.

4. In cases, also, where the pressure of the enlarged veins on the spermatic nerves produces repeated attacks of spermatorrhœa; and these, cases are by no means uncommon. These are, however, more frequently met with out of the hospital than in individuals of the class who apply to such institutions for relief. In fact, young men of the more highly-educated classes are very subject to it, especially those who habitually lead a sedentary and studious life, as, for instance, young clergymen and lawyers. In these persons a peculiarly hypochondriacal state is brought on by the tendency of the mind to dwell on the condition of the genital organs, and the patient is constantly fidgetting about the local and tangible disease he observes in them. This was the case in the patient on whom I operated a short time ago for double varicocele, and who has received a better education than most hospital patients, for he belongs, in some degree, to the medical profession. His anxiety with regard to this disease, though the veins affected have been perfectly occluded by the method presently to be described, is still so great that, at his earnest importunity, I removed, last week, a portion of the scrotum which happened to be rather more pendulous than is usual, partly in order better to support the testis, and partly that his morbid feeling on this subject might in some degree be assuaged.

Now, how should the radical cure of this condition be produced? To this I would answer—By exciting adhesive inflammation of the spermatic veins, through an application of the same principle which sets up that process in the veins of the lower extremity. There are several different ways of doing this; some are very objectionable. The twisted suture, as applied to the veins of the leg, induces too great irritation in the scrotum, and there its introduction is often followed by violent inflammation or sloughing, or by an opening up of the cellular tissue of the scrotum with œdema, and even purulent infiltration. It is better, I think, not to use this method here, indeed I have twice, in the practice of others, seen it followed by death. The plan I have adopted for some years, is that suggested and practised by Vidal, a distinguished French surgeon, and is as follows:—The vas deferens, readily distinguished by its round cord-like feel, is first separated from the veins, and entrusted to an assistant; next an iron pin, bored with a hole at each end, is passed between the vas and the veins, and brought out, first notching the scrotum with a scalpel at the point of perforation; then a silver-wire, threaded on a needle so constructed that the wire shall follow it without catching, is passed in at the aperture of entry of the needle, and then carried between the integument of the scrotum and the veins, the wire is brought out at the second puncture. Each end of the wire is now passed through the corresponding hole of the pin, which is twisted round and round repeatedly, each turn causing the wire to be rolled around the pin, and so tightened till the veins are firmly compressed between the pin behind and the loop of wire in front. By this means the scrotum is quite free and uncompressed, and there is no danger of arousing inflammation or œdema. The wire should be tightened from day to day, as it causes ulceration in the veins until it has completely cut through, which results, usually, in about a week or ten days. Meanwhile there is much plastic matter thrown out around the veins, this finally contracts, and obliterates their channels. This method is an effectual and permanent cure, as we had an opportunity of seeing in the case of a porter at this hospital, on whom I performed this operation with perfect success, for he remained here for three years after the operation, during which time he was perfectly free from any return of his disease. Of late, I have been in the habit of employing a simpler method. I separate the vas in the usual way, and then make a small incision, about half an inch long, in the front and back of the scrotum, afterwards passing a needle armed with silver-wire, as before described, between the vas and the veins, bringing it out behind, then returning the needle, but this time carrying it in front, between the veins and the skin, and so including the veins in a loop of wire, without implicating the scrotum. This is then tightly twisted together, so as to constrict the inclosed vessels. This plan had a similar effect to that of the wire and pin combined; by repeated tightening the wire gradually effected a passage, by ulceration, through the veins, which were obliterated by the same process.

It has been objected to this and similar operations, that atrophy of the testis may take place from its arterial branches being included together with the veins, but as the spermatic artery runs near to the vas deferens, and is held out of the way with that duct, it escapes, and the chance of that mischief is avoided. Nevertheless, atrophy of the testis may coexist, as the result of long-continued pressure of the blood in the vessels of the gland before the operation was performed. —*British Med. Journal, Feb.* 25, 1860, *p.* 141.

ORGANS OF RESPIRATION.

68.—ON A CONVENIENT INSTRUMENT FOR EXAMINING THE BASE OF THE TONGUE AND EPIGLOTTIS.

By P. C. Price, Esq., M.R.C.S., Surgeon to the Great Northern Hospital, &c.

In cases of disease of the base of the tongue and parts immediately in relation with the epiglottis, much difficulty is often experienced in endeavouring to obtain a clear view of the parts involved, while it is often impossible even to catch a glimpse of the morbid condition. When such is the case, the surgeon is compelled to remain content with the conclusion at which he may arrive by the symptoms exhibited. To facilitate the examination of the upper part of the throat, I am in the habit of using a little instrument, which consists of a small disc of hard steel, very highly polished, and attached to a slender rod of soft, bendable, metal, by means of a ball-and-socket or screw joint, which gives great freedom of movement, and allows the plate of metal to be placed at any angle with its stem. The polished disc acts as a mirror of the first class, and when raised somewhat above the temperature of the breath (by being dipped into hot water), enables the surgeon to detect at the base of the tongue, and about the epiglottis, such morbid conditions as excoriations, ulcerations, small tumours, &c.

This simple contrivance was made for me by Mr. Matthews, of Portugal-street, and is much used, I believe, on the continent. In two instances of syphilitic ulceration of the throat, lately under my care in the Great Northern Hospital, it proved of considerable advantage; and its general utility will, I have but little doubt, be duly appreciated on many occasions.—*Lancet, Dec.* 24, 1859, *p.* 643.

69.—*On Tracheotomy.* By E. Lawford, M.D., Leighton Buzzard.—[One great difficulty in performing tracheotomy is that experienced in preventing the trachea from rolling under the knife. This is particularly experienced in young subjects, where the adipose tissue is abundant, and the trachea more deeply situated.]

Instruments have from time to time been introduced to the profession, calculated to overcome this difficulty. One surgeon has advised a tenaculum to be used to elevate the tube. Another directs that the patient should be requested to perform the act of swallowing; and, at the moment when the tube is elevated and fixed by this action of the muscles, the favourable moment should be seized, and the scalpel immediately introduced. This would be all very well, provided the patient will obey directions; but children and half-asphyxiated people will not or cannot obey such directions, and we must do that which is necessary to be done for them. Now, it strikes me that there are three requisites in an instrument for the safety and expedition of this operation:—1. A transfixer, to steady the larynx; 2. A guard against the effects of antero-posterior pressure; and 3. A director for the knife. I say a guard against antero-posterior pressure; for I maintain that, in young subjects, the pressure necessary to carry the knife through the elastic rings would bring its point in dangerous proximity to the posterior wall; so much so that I think it is positively unsafe to open it without a director.

[The cassigna hook in common use in the French hospitals transfixes and elevates the trachea; but, inasmuch as the groove for the knife is on the convexity of the curve, it does not guard it, neither does it support the ring to be divided. Dr. Lawford recommends an instrument essentially similar to this, but with the groove for the knife on the concavity. This instrument answers the objects to be held in view, viz., transfixing the larynx, preventing antero-posterior pressure, and guarding the knife. This instrument may be obtained of Mr. Coxeter, of London.]—*British Med. Journal, Nov.* 19, 1859, *p.* 943.

ALIMENTARY CANAL.

70.—ON THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

By John Wood, Esq., F.R.C.S., Assistant-Surgeon to King's College Hospital. (Read before the Royal Medical and Chirurgical Society.

[The plan followed by Wutzer and other operators fails because of their not obtaining a hold upon the posterior wall of the canal, and their securing only the anterior portion of the fold produced by invagination. Moreover, the principle of plugging up a dilatable orifice, like that of the inguinal opening, is a false one.]

The principle of Mr. Wood's operation is directly opposite to that of dilatation,—namely, that of drawing together and compressing the anterior and posterior walls of the canal in its whole length, and their union by the adhesive process with the invaginated fascia of the scrotum, which is detached from the skin and transplanted into the canal, the skin being left to adhere below to the approximated margins of the external abdominal ring. By this means the posterior wall of the inguinal canal is made to act as a valve to prevent any future descent of the bowel, shutting up the superior opening by becoming united to the anterior wall through the medium of the scrotal fascia, which thus affords a very highly organized and vascular connective tissue between the tendinous surfaces, which it would be very difficult to cause to adhere together otherwise. The fascial invagination becomes likewise firmly adherent to the spermatic cord. This continues to be effective, even when the temporary effusion of lymph is reabsorbed.

The operation.—This consists, 1st, in detaching the scrotal fascia from the skin over the lowest part of the hernial protrusion with a tenotomy knife, and then invaginating the fascia into the canal with the forefinger ; 2ndly, in passing a strong, well-curved-needle, fixed in a handle, armed with a stout, thick thread, and guided by the finger through three points in the canal—viz., the conjoined tendon and the triangular fascia (forming the posterior wall), and the external pillar of the ring, close to Poupart's ligament, (forming the anterior wall of the canal). The ends of the ligature are left in the two former punctures, and a central loop in the latter, passing through the pillars of the external ring, and through the same aperture in the skin of the groin. This may readily be done by sliding upon the subjacent aponeurosis. 3rdly, a cylindrical or flattened compress of glass or box-wood, two inches and a half long by one inch wide, is tied firmly upon the axis of the canal by passing the ends of the ligature through the loop, and tying over the compress. Before tightening the ligature, the surgeon should satisfy himself, by passing the forefinger through the external ring, that the ligatures draw upon the posterior wall. The opening in the scrotum should be tucked well up to, but not within, the external ring.

In recent cases of hernia, in which the sac is small and possesses an intimate vascular connexion with the peritoneum, and a very slight one with the cord, it may be pushed back into the superior opening, and the ligature applied altogether external to and without puncturing the sac, thus diminishing very much the chances of peritoneal inflammation. But in old and large herniæ, the sac has a more intimate vascular connexion with the scrotum and cord, and constitutes, as it were, a separate structure, distinct from the peritoneum. In these cases the sac is necessarily invaginated with the fascia, and the ligatures pass through it. In these the inflammation set up in the sac is much less liable to spread into the abdominal cavity, especially when the upper orifice is closed by the ligature. In a large sac the adhesive process is necessary to complete obliteration of the canal, and to prevent future complications.

The compress is removed from the fourth to the seventh day, according to the degree of action set up. The ligatures may be left in a week or two longer to act as conductors for the discharges, and to keep up consolidating action as long as may be desirable. When the sac is punctured, serous fluid flows from the wound in greater or less quantity during the first three or four days.

The author called attention to the action of the rectus muscle upon the inguinal canal, through the conjoined tendon, in drawing backward the posterior wall of the hernial canal, thus aiding the dilating action of the protruding bowel in the production and growth of the hernia. The effect of the ligatures and consequent adhesions in his operation directly counteracts this action of the rectus. He considers that the first tendency to oblique inguinal hernia, so often hereditary, is owing to deficient development of the lower fibres of the internal oblique producing an imperfect covering to the internal ring. In some of the cases operated on, he has succeeded in supplementing this deficiency by passing the scrotal fascia well up in front of the internal abdominal ring, and securing it to Poupart's ligament in that position.

He considers that the chief source of failure in the performance of his operation, especially in large and old cases, is in not securing a hold upon the posterior wall. By simply attaching the fascia to the pillars of the external ring, and drawing the latter together, the hernia, though prevented for a time from descending into the scrotum, still occupies the canal, and will, sooner or later, again dilate the external ring, unless constantly bolstered up by a truss. The closing of the external ring by the lower ligatures, in this operation, contributes much, however, to secure in its new position in the canal the transplanted fascia.

In small cases of direct hernia, the closure or obliteration of the external ring only may be effective in producing a cure, if care be taken to obtain a hold with the inner end of the ligature upon the triangular fascia covering the border of the rectus, immediately behind the opening of the external ring.

In noticing the objections to the plan, the author showed that, by properly protecting the point of the needle with the finger, and keeping in front of the fascia transversalis, all danger of wounding the epigastric and circumflex iliac vessels or the bowel was guarded against.

The fear of peritonitis is avoided in recent cases (in which it is most to be dreaded), by not puncturing the sac at all, closing up the tendinous opening external to it. In old cases, adhesive action may be set up in the sac without fear of its spreading to the peritoneum, as the results of numerous cases have shown. The objections made to the limited incision into the skin of the scrotum (which is little more than a puncture) he considers to be puerile. Its advantages in permitting the escape of discharges are evident.

Full reports of fifteen cases of hernia (all inguinal) were appended to the paper. One of the cases was a boy of eight years of age; the ages of the others ranged from fifteen to fifty-four and fifty-eight years. One was a female with bubonocele; the rest were males. Three were cases of direct, the rest of oblique hernia. Thirteen were scrotal; four of large size, and three with very large and lax internal openings. Two were congenital, and two complicated with varicocele (cured also by the operation). In only one case were the symptoms at all severe, or gave suspicion of peritonitis. In this case, the patient was in King's College Hospital eight weeks; the symptoms were produced by burrowing of matter between the oblique muscles, following a diarrhœa then prevalent in the hospital (in July last). This patient made an excellent cure, was treated entirely without truss, and was one of the cases shown to the Society. The hernia had a very large internal opening, and the subject was cachectic and ill nourished before the operation. In one other case, the patient was in bed a month; in another, there was partial sloughing of the sac, which was a large and long one, with a very pendulous scrotum, and a large varicocele. This case was treated also entirely without truss, and both hernia and varicocele were cured in eighteen days. The duration of treatment in the rest of the cases varied from nine to twenty-one days. Eight were treated entirely without truss. Thirteen are good and persistent cures, and have remained firm ever since, extending over the following periods of time: one (the first), very nearly two years,—this case was published in 'The Lancet' of the 29th of May, 1858; another, one year; two, ten months; four, nine months; three, eight months; one, two months. Three of the cases had been before operated on by Wützer's and Ragg's methods; one case was operated on twice; one is doubtful; one was reruptured by indiscreet and early hard lifting without truss.

Six cases of cure were exhibited by the author to the fellows of the Society. Of these, four had been treated entirely without truss, and all had been well, and some severely, tested by lifting and heavy labour. The first case (operated on nearly two years ago) was amongst those exhibited. No difference whatever was apparent between the groins of the two sides. One had been cured a year, three nine months, and one eight months. One of those treated without truss was congenital, in a young man aged twenty years; another was of five years' standing, in a man aged fifty-eight. The rest were of eighteen, sixteen, and three months' standing respectively. All were scrotal herniæ, and two direct. Two had chronic bronchitis (at times severe) after the operation, and one during the progress of the cure.

[In the discussion which followed the reading of this paper, Mr. HENRY LEE suggested another mode of operating, which he believed would attain the same results by less complicated means.]

The operation which he suggested consisted in invaginating the skin of the scrotum into the inguinal canal with the forefinger of the left hand. Using, then, the finger as a guide, the needle, armed with a ligature, was passed through the external wall of the inguinal canal, and out through the skin of the abdomen; one end of the ligature was then left free. The needle, being withdrawn, was then passed through the same aperture in the scrotum, and through the internal wall of the inguinal canal out through the skin. The needle then being taken away, the two ends of the ligature were tied together. The ligature was then allowed to remain for several days, or to ulcerate its way out. He considered that this plan had several advantages. It dispensed with the necessity of any dissection of parts, and of the dangers arising therefrom. The skin of the scrotum descended into its natural position two or three days after the operation, and there was less consequent deformity. Although the sides of the inguinal canal were brought together by the ligatures, yet the permanent adhesion depended upon the lymph effusion around the ligature, which was allowed to remain for that purpose. Another advantage was, that if any matter formed in the course of the punctures made, the presence of the ligature would ensure a ready outlet for the fluid. He (Mr. Lee) related the particulars of a case in which he had performed this operation in King's College Hospital, in the middle of last September. The patient, a butcher, accustomed to heavy work, had continued his occupation for several months without any truss, and with the inguinal canal quite closed.

Mr. Curling had no doubt that a more simple mode of procedure than that described by Mr. Wood might be adopted in many cases with success. A case of omental hernia had lately been published in the journals, in which he had employed the principle practised by Mr. Wood, but in a manner somewhat similar to that followed by Mr. Lee—by passing the needle through the anterior and posterior walls of the canal. The proceeding was quite successful. Mr. Wood was entitled to great credit for the research and originality he had exhibited in his treatment of the subject before the Society. Surgeons for a long time past had been endeavouring to find means of overcoming the difficulty of obtaining a radical cure for hernia. In no case, however, had they gone to the root of the evil as Mr. Wood had done—viz., the contraction of the opening through which the intestine descended. He (Mr. Curling), however, did not see the necessity of separating and invaginating the superficial fascia. He thought Mr. Wood's object would be attained in the passing of the needle by cutting through the superficial fascia, and carrying the finger onward into the canal. If Mr. Wood's operation were ever resorted to extensively, it must be for the purpose of dispensing with the truss, which was an object of great importance, especially to poor persons. In the cases before the Society, an operation of a very difficult character had been skilfully performed, and it would be hardly fair to expect that in other hands the results would be so favourable. Previous to the performance of the operation which he (Mr. Curling) had alluded to, he thought it desirable to practise it upon the dead subject. He accordingly did so on that of a phthisical subject, in which the inguinal canal was constricted. Now, in passing the needle under the conjoined tendons, he included a small portion of peritoneum. Such an occurrence, even in skilful hands, might possibly happen when the operation was performed upon the living body. From what he had heard from Mr. Wood, he understood that in many of his cases the sac was invaginated; the ligature, therefore, passing through the sac must pass also through the membrane continuous with the peritoneum—a proceeding which would not be altogether without danger.

Mr. Holthouse thought, although the operation performed by Mr. Wood was a valuable one, that too much importance had been attached to the drawing together of the canal and the orifices. He considered that the main part of the cure must be attributed to the adhesion of the opposed surfaces of the canal. If this were really the case, it might be questioned how far an operation was applicable in every instance of the affection. Hernia presented itself to us in great variety, and the operative proceeding, in order to ensure success, should be adapted to the peculiarities of each case. In Gerdy's operation cures had resulted, not as the effect of plugging up the canal, but of the adhesion of its sides. In some cases which had come under his observation, the plug had come down very shortly after the performance of the operation, but a radical cure was, nevertheless, effected. This could only have resulted from the adhesion of the opposite walls of the canal. With respect to the occurrence of peritonitis after Gerdy's operation, he had examined sixty-two cases in which it had been performed. Amongst these, four deaths had occurred, but not one of these was from peritonitis. One death took place from pyæmia, one was accidental, one from suppuration in the abdominal walls, and in one the cause was unknown. In a case which had come under his (Mr. Holthouse's) care, in which Mr. Wood's operation was performed, great pain was suffered in the abdomen for the first three or four days, and fears of peritonitis were entertained. On removing the ligatures, however, at the end of the fifth day, the pain gradually subsided, and the patient made a good recovery. He died of bronchitis about four months afterwards, and no traces of peritonitis could be detected.

[Mr. Spencer Wells observed that Wützer's operation was in many cases preferable to either Mr. Wood's or Mr. Lee's.]

The danger of passing the needle through the sac was small in comparison with that of introducing vegetable threads, which had the effect of strangulating and killing the portions of tissues which they encircled. In one case, at Chatham, he believed, death had resulted from the performance of Mr. Wood's operation; another fatal case had occurred at King's College Hospital; and one case, in which Mr. Wood had himself operated, had been attended by symptoms of a very serious character. In cases in which the canal was long and narrow, and where the plug could be introduced with facility, the simpler and safer operation of Wützer was to be preferred. Where, however, the canal was short, the abdominal rings wide, and it was only possible to apply constriction upon one point of the canal, the operation of Mr. Wood, or some modification of it, such as that suggested by Mr. Curling, might be employed with advantage.—*Lancet, March* 10, 1860, *p.* 246.

71.—RADICAL CURE OF HERNIA; A NEW MODIFICATION OF WUTZER'S OPERATION.

In the following case, in which a new modification of Wützer's operation was performed, his instrument was wholly dispensed with, and the sides of the canal were brought together by a simple ligature.

The ligature was allowed to remain and to ulcerate its way out, in order that any matter that might form should have a free exit. The consolidation of parts is produced not by the invaginated tissues so much as by the lymph effused around the ligature in the inguinal canal. Mr. Lee considered this modification of Wützer's operation safer than some others lately introduced, inasmuch as it involved no separation of the skin from the subjacent tissues, and was consequently less likely to be followed by cellular inflammation. The plan of allowing the ligature to ulcerate its way out Mr. Lee also thought was attended with a twofold advantage,—namely, 1st, it ensured an escape of any matter that might form in its course; 2nd, its prolonged presence in the inguinal canal rendered the obliteration of that canal by the effusion of lymph more certain.

Benjamin F., aged sixteen, was admitted into the hospital on the 17th of September, 1859, with an oblique inguinal hernia of the right side. The hernia descended every time the patient stood up, but was again easily reduced. Below the external ring there was a permanent enlargement, caused by a hydrocele of the cord. This was quite distinct from the swelling produced by the hernia. On the day of admission, the skin of the scrotum was invaginated into the inguinal canal, and a ligature was passed, by means of a curved needle, along the invaginated portion of skin, through the internal wall of the inguinal canal, and out through the skin of the abdomen. The needle was then withdrawn, leaving one end of the ligature protruding through the opening. The needle was then again introduced, by the same opening into the invaginated skin, through the external wall of the inguinal canal and through the skin of the abdomen. One extremity of the ligature was left free, projecting through this opening, and the needle was withdrawn. The two ends of the ligature were now tightly tied together, bringing into conjunction the opposite sides of the inguinal canal, and embracing a portion of the superjacent skin. The needle, in being introduced, was purposely made to pierce the walls of the hydrocele of the cord, and some transparent fluid escaped. No disturbance of any kind followed the operation.

Sept. 22nd. The invaginated portion of skin had descended to its natural position.

24th. The ligature was tightened.

25th. A considerable amount of pain produced by the ligature; general health very good.

26th. Pain relieved; there is now no impulse on coughing in the inguinal canal; the wound discharges freely.

Oct 4th. There was a recurrence of the pain in the situation of the ligature during the night.

6th. The pain had subsided; no constitutional irritation.

17th. The ligature, being held by a piece of tendon or fascia, was removed; there is no impulse on coughing.

Nov. 4th. Left the hospital a day or two after the last report, and has been at work since that period, wearing a truss; there is no impulse upon coughing; the external wound is healed; there is no mark on the scrotum where the ligature passed. The hydrocele of the cord had been apparently quite cured. He was directed to discontinue his truss.

Ten days later, he again presented himself, having continued his work without a truss. The pillars of the external ring could be felt separated, but above this the inguinal canal appeared quite closed.

Dec. 5th. This patient again presented himself, having continued his work without a truss. The hernia appeared perfectly cured.—*Lancet, Jan.* 14, 1860, *p.* 35.

72.—ON A NEW MODIFICATION OF WUTZER'S INSTRUMENT FOR THE RADICAL CURE OF REDUCIBLE HERNIA.

By Furneaux Jordan, Esq.. Assistant-Surgeon to the Queen's Hospital, Birmingham.

In Wutzer's operation, the difficulty which all surgeons have experienced and described, is in obtaining sufficient adhesion between the invaginated cylinder of integuments and the posterior wall of the inguinal canal, and in efficiently blocking up the inner inguinal aperture. In two cases which were recently under my care at the Queen's Hospital I used the following instrument with the best results.

The instrument consists of a hollow metal plug. In the interior, and along the upper wall, is a small canal for the needle; beneath the needle, 'and occupying the remainder of the hollow space, is a vulcanised india-rubber apparatus of a pyriform shape, the wider extremity being at the round end of the plug. At the under aspect of the tubular plug is an aperture, oval in form—one extremity being a little wider than the other—seated close to the end of the plug, and slightly encroaching on its rounded extremity. Through the aperture dilatation may be effected with air, or water, or glycerine—fluid is perhaps preferable, because however accurate the stop-cock, probably a little air may escape—by means of a small syringe, which can be easily applied to a tube and stop-cock connected with the smaller extremity of the vulcanised tube, and fixed to the flat end of the instrument. The tube must be introduced into the hernial canal, the needle passed, and the skin-piece fixed, *before* dilatation is effected. The attempt to dilate before fixing the plug would result in the repulsion of the instrument to an extent corresponding with the degree of dilatation. The amount of fluid dilatation of the instrument, of which it is desirable to have three sizes, admits of easy measurement before it is used. The instrument may be adapted to almost every form of hernia (the cases in which I used it were direct inguinal), by bending or otherwise altering the form of the tube, or by directing the fluid dilatation at will by an aperture in any part of the plug, or by uniting both these possible methods of modification.

It advantages are these:—The fluid dilatation posteriorly presses upon the posterior wall of the inguinal canal, and thus induces adhesion between the approximated sides of that canal and the posterial aspect of the invaginated mass of cutaneous structures. But it does more than this: *the air or fluid pressure, from its peculiar character, is exerted chiefly at and fills up the internal ring or other aperture—whatever its position, or shape, or size.*

I would suggest here that—as *continued* pressure is the most effective agent in the absorption of inflammatory exudations and the atrophy of normal structures—the pad, spica bandage, and subsequent truss, might perhaps be omitted with advantage in the after treatment of the radical cure of hernia, the patient simply wearing an efficient suspensory bandage. We want as much new and old tissue as possible in the inguinal apertures. To take great pains to excite inflammatory exudation, and then to take equal pains to disperse it, is surely a surgical paradox.—*Med. Times and Gazette, Jan.* 21, 1860, *p.* 58.

73.—*Injections of Iodine into the Tract of Fistulæ in Ano.*—Many patients refuse the knife in this complaint. Hence many contrivances have been devised; amongst which, Mr. Luke's instrument for gradually cutting through the parts by means of a thread and rack has been found very useful. No means but the knife, or such substitutes as Mr. Luke's instrument, can be of any avail when the tract has become callous and insensible to stimulating or caustic injections; but, in recent cases, various injections have proved of great service. Of late, pure tincture of iodine has been tried; and we find several successful cases related in 'L'Union Médicale' of the 26th ult., by M. Henry, assistant to M. Bonnafont, at the Infirmary of the Invalides (the analogue to our Chelsea Hospital) in Paris. To prevent the injections irritating the rectum, in complete fistula, M. Bonnafont places a tent in the bowel, reaching about an inch above the intestinal orifice of the fistula. This tent is also very useful in respect of diagnosis, when we are in doubt whether the fistula is blind or complete. Put the tent into the rectum pretty high up, and inject iodine; if it comes out unstained, the fistula is blind; if stained, it is complete.—*Lancet, Dec.* 17, 1859, *p.* 621.

74.—*Severe Prolapsus Ani; Sloughing of the Mass; Cure.*—In old prolapse of the rectum, where considerable hypertrophy and induration of the protruded part have occurred, it is commonly necessary, when great inconvenience arises, and the return is not to be effected by supine posture, cold applications, and equal pressure, that the prolapsed part be cut off at the base, or treated with nitric acid. In a case of severe prolapsus of the rectum admitted into St. Mary's Hospital, under the care of Mr. Coulson, any such proceeding was superseded by the operation of nature. The patient, an old man of sixty-two, was the subject of an old prolapse which had been irreducible, and was very painful during the week prior to his admission. When admitted, it existed as a circular tumour, lobulated, turgid, hard, and inflamed, of the size of a small orange. It was uncomplicated with hemorrhoids; and there was no retention of urine. The patient suffered considerable dragging pain, and defecation was highly inconvenient. Emollient applications, laxative medicine, and the supine position were employed to relieve his pain. Sloughing of the tumour, however, quickly ensued after his admission, and in the course of a week the whole plug of membrane sloughed away, and a complete and spontaneous cure took place. The cure in such a case results from a precisely similar process to that which the surgeon imitatively excites by the employment of the ligature. Artificial strangulation of the base of a hemorrhoidal tumour has, however, this advantage for the patient over the natural process seen in the above case, that in the latter instance the strangulation is necessarily preceded by a considerable amount of inflammation and engorgement, which are productive of great pain and suffering during the preliminary stages.—*Lancet, Nov.* 26, 1859, *p.* 535.

ORGANS OF URINE AND GENERATION.

75.—ON LITHOTOMY CONSIDERED AS A CAUSE OF DEATH.

By Professor A. Buchanan, M.D., University of Glasgow.

[To simplify the subject as much as possible, Dr. Buchanan limits the present paper to a consideration of the causes of death in cases where the organs are sound and healthy, and the general health good.]

There are, according to my views, four causes which render the operation for stone in itself dangerous or fatal. These are—1. Infiltration of urine into the subserous cellular tissue of the peritoneum, and the passage of it through that membrane, by osmosis or simple percolation, into the interior of the peritoneal cavity. 2. Infiltration of urine into the subfacial cellular tissue of the pelvis, while all access to the peritoneal cavity is debarred by the dense fibrous membrane. 3. Mechanical contusion of the bladder and parts adjacent, of which what is called "shock" is the most severe form. And 4. Hemorrhage.

1. It may be objected to this arrangement that the two first causes are comprehended under the general expression, infiltration of urine.

But it is the very generality of that expression, as employed to indicate a cause of death after lithotomy, that renders it objectionable, from its being ambiguous and devoid of definite meaning. Infiltration of urine is not in itself a cause of death, for it occurs after every operation for stone, but only infiltration in certain situations; and unless these situations are specified, no accurate idea has been expressed. That mere infiltration of urine is not a cause of death, we have evidence in what we see occur every day in urinary abscesses, or effusion of urine under the superficial fascia of the perineum. That the urine must be largely absorbed after such effusion is certain, but we never see any bad effect thence resulting; nor apprehend anything more than the local mischief arising from the irritant action of the urine, which occasions sloughing of the cellular tissue, and that may be so severe and extensive as to cause death, especially if not mitigated by incisions. Such external effusions warn us what must be the extreme danger of effusions of urine proceeding inwardly into situations where there is no possibility of its escaping, and where it comes into contact with parts more susceptible of irritation. Still, however, the affections induced, whether by external or internal effusion, proceed alike from a local irritation. But such affections are so totally dissimilar in respect of the symptoms which they exhibit and the course which they run, according to the seat of that irritation, that no man who has any pretension to scientific accuracy in his discrimination of diseases can possibly confound them together; which is clearly done when they are referred indiscriminately to infiltration of urine.

The two forms of urinary infiltration, which become causes of death after lithotomy, take place in consequence of special errors in the mode of cutting for stone, which it is most important to signalise. The first kind of infiltration has been observed chiefly after the rectangular operation, and was, indeed, never discriminated as a distinct affection till that operation came into use. It may, however, occur readily after the common lateral operation; after which, however, the second form of infiltration more frequently presents itself. These specialities depend upon the particular errors into which the operator is most apt to fall in adopting these respective modes of operating.

Six cases which I have described present no illustration of the second kind of infiltration, and all of them but one seem to me to be instances of the first. Of these cases, I have described minutely the appearances that presented themselves after death in the second, third, and fourth. The two first of them, notwithstanding the care with which I examined and reflected over them, completely baffled me, and left my mind in a state of utter perplexity and darkness with respect to their true nature: the only point which admitted of no doubt as to both, being that the patients had been, in some unascertained and mysterious way, stabbed in the lower belly. It was only on examining the third case that daylight broke in upon me, and gave me a glimmering idea of the probable nature of them all. An accidental circumstance, which I have mentioned above, first put me upon the right path. The ecchymosis, described above, terminating in a well-defined line on the sigmoid flexure of the colon, attracted my attention. The blood forming it had obviously come from below. I traced it all round the rectum, where that intestine is covered with peritoneum, and beyond the reflection of the peritoneum I found the blood extending downward, between the rectum and the bladder, as far as the external wound. The source of the blood was thus obvious. From the external wound it had made its way into the cellular tissue between the bladder and rectum, till it reached the reflection of the peritoneum (recto-vesical pouch), and thence, following the course of that membrane, it had been diffused alike over the bladder on the one side, and the rectum and colon on the other.

But granting this to be the true origin of the blood, it will be said that blood is an innocuous fluid; and that the effusion of blood under the peritoneum never could give rise to the grave symptoms under which those patients laboured. It must, however, be considered that, wherever blood passes, a more subtle and penetrating fluid will pass more readily and extensively. The urine, therefore, with which the surface of the wound after lithotomy is continually bathed, entering by the same aperture as the blood, will diffuse itself in the cellular substance between the bladder and rectum, and will thence make its way into the subserous tissue behind the peritoneum covering the parietes and viscera of the abdomen. But will it stop behind the peritoneum? Assuredly not. It will pass through that membrane by osmosis, or by simple percolation; and will enter the peritoneal cavity. That the sanguinolent fluid, which was in four out of five of these cases found in the cavity of the abdomen, consisted of urine holding the colouring matter of the blood in solution, I entertain no doubt; although it was not proved to be so by chemical tests. But, if urine be infused into the cavity of the peritoneum, it will produce the same effects, whether it enter by the circuitous route just described, or be directly effused from a rupture of the urinary bladder. Now, the known effect of the effusion of urine, of bile, or of the contents of the alimentary canal into the peritoneal cavity is death in about twenty-four hours; and I refer to the same cause the fearfully-rapid death that takes place after a badly-performed rectangular operation for stone.

Such is my view of these rapidly fatal cases, and there seems to me to be two causes of the rapidity of the fatal event. First, the rapidity of the infiltration, taking place from a wound situated immediately beneath the open urethra, along which the urine secreted into the bladder continually passes to be diffused laterally over the cut surface, and the orifice of the open wound beneath the prostate; and, second, the circumstance that the urine passing inwardly by a wound beneath the prostate is not confined by any fascial covering, but comes into immediate contact with the peritoneum.

This view of the subject is to my mind, at least much more probable, as being more consonant with facts and with physiology, than the theory which ascribes to shocks all deaths that occur rapidly after lithotomy. How, it may be asked, can such a cause be assigned in such cases as No. 3, in which the whole duration of the operation was less than a minute, and the wound was of small size?

2. In the absence of cases illustrative of the mode in which death takes place after the second kind of urinary infiltration, we must, for the purpose of comparison, borrow the descriptions which have been given of them. The patients, instead of being seriously affected immediately after the operation and dying in twenty-four hours, are described as sometimes appearing to do well for several days, when they rolled round upon their bellies, and died in great agony; or, as sometimes surviving for a longer period, with gradually increasing emaciation, or hectic symptoms: while after death there is found purulent infiltration into the cellular tissue of the pelvis, or circumscribed abscesses. There is, therefore, a total dissimilarity in these cases to the former, whether we look at the symptoms during life, or the pathological lesions after death. We may infer also from the kind of operation performed, that the wound extending beyond the rim or base of the prostate had divided the deep perineal fascia, or levator ani muscle, in such a way as to permit the infiltration of urine under the recto-vesical fascia where it lines the sides of the pelvic cavity.

The views stated above as to the cause of the infiltration of urine have a most important bearing upon the operative process in lithotomy. If it be true that the greatest danger of lithotomy arises from urine getting into the cellular space beneath the prostate, or rather between the bladder, prostate, and urethra on the one side, and the rectum on the other, then the great rule for performing lithotomy safely, is to avoid carefully this hypoprostatic space, as I shall take the liberty of calling it. This space is enclosed by strong and well-defined boundaries. It lies just beneath the recto-vesical pouch of the peritoneum, and is therefore conterminous with the abdominal cavity, from which it is separated by a thin membrane. It has the bladder, prostate, and a small portion of the membranous part of the urethra in front; the rectum behind; and on each side the recto-vesical fascia, or lateral ligament of the bladder. The vas deferens and the vesiculæ lie within it, and it is filled with a delicate cellular tissue free from fat. When the bladder is folded forward over the pubes, the hypoprostatic space has very much the shape of a four-sided pyramid, of which the apex is truncated, and lies a little below the apex of the prostate. The boundary forming this apex consists of the deep perineal fascia where it intervenes between the urethra and rectum, and forms there the only fibrous septum for the protection of the abdominal viscera. If this septum be divided, while the bladder is at the same time opened, infiltration behind the prostate must necessarily ensue.

It is of great importance, therefore, to study the boundaries of the hypoprostatic space, with the view of determining at what points they are most apt to be violated by the knife of the lithotomist. I am satisfied that the great source of the danger of lithotomy has been generally misunderstood. We may see this in the admonition invariably given to the student and young operator never to extend his incision beyond the base of the prostate. This admonition, in so far as it represents such an incision as the great source of danger in lithotomy, involves a double error; for there are certain directions in which the base of the prostate may be both safely and advantageously cut; and there are other directions in which a fatal error will be committed, although the gland is not divided half way to the base. The just and true admonition, which I would substitute for the above, is never to do the slightest violence to the fibrous tissues, which form the three planes at the apex and the two sides of the pyramidal space beneath the urethra, prostate, and bladder.

The mode and place in which the hypoprostatic space is laid open, differ in the old operation and in the new. In the latter the lateralized knife, penetrating too low, divides the deep perineal fascia behind the urethra; and being thought to be in the groove of the staff, it is carried deep under or through the substance of the prostate into the lax cellular tissue beyond. The lateral incision to the left side completes the violence inflicted on these delicate parts. The operator now ascertains with his finger that he has not entered the bladder, and rectifies, as he supposes, his mistake, by carrying his knife higher up and dividing the prostate; but it would be much wiser in such circumstances to send the patient to bed after the first unsuccessful thrust: for the wound, though a severe one, is not yet necessarily fatal, but by the second incision, infiltration of urine into the subserous tissue of the peritoneum, and death in twenty-four hours becomes inevitable. This is, I believe, the true explanation of the frightful rapidity of these fatal cases. They constitute a new phase in the history of lithotomy, altogether peculiar to this new operation. Five out of six fatal cases that have occurred in our hospital have been of this kind, and cases in every respect similar have occurred in private practice. This is now so generally known in this city, that an operator, if his patient is not dead or obviously dying at the end of twenty-four hours, is congratulated on the success of his operation.

There are two other modes in which the hypoprostatic space may be laid open in performing the rectangular operation for stone, which I think it important to signalise. It is well known that in whatever part of the urethra or prostate the angle of the staff be made to rest, that part is protruded in front of the orifice of the groove projecting in the perineum. If, therefore, the operator be not careful to ascertain that the angle of the staff is sufficiently far forward, if he allow it to rest at the back of the membranous portion of the urethra, the deep perineal fascia is folded over the mouth of the groove, and must be divided in entering the groove with the knife; and if the angle rest on the prostatic portion on the urethra, on the body of the gland, or farther back, a still deeper wound will be inflicted on entering the groove, as it is easy to verify on the dead body.

The third mode in which I have seen the accident happen, is the result of a mode of operating, which some operators have tried, but which, as appears to me, is not to be recommended. They make the external incision first, that they may enter the groove of the staff with more facility by a second incision, But in thrusting the knife into the middle of the perineum for the purpose of commencing the first incision, the deep perineal fascia, which is only a few lines from the surface, must be very liable to injury, as the knife can enter the middle of the perineum in no way safely unless lodged in the groove of the staff. The wound thus produced may not be very deep, but I have seen the most formidable symptoms result from it—long-continued shivering, profuse sweating and diarrhœa, lasting for several days, and causing great anxiety for the patient's safety, although he ultimately did well.

In the lateral operation, from the direction of the incisions they are less apt to do injury to the hypoprostatic space, and cannot do it to the same formidable extent; but that space must be laid open more or less by those misdirected incisions that penetrate into the rectum and injure the vesiculæ seminales. It seems to me at least not improbable, that in many of the rapidly fatal cases, death may be owing to this cause rather than to shock, to which it is usually ascribed. The more the incision of the prostate is directed downward, the accident must be the more likely to happen. Perhaps, therefore, we may thus explain the increased mortality after the lateral operation of lithotomy, which seems to me beyond all doubt to have taken place in our own day. Cheselden cut the prostate outward, keeping the edge of his knife turned up. But we have since been recommended, upon high surgical authority, to divide the prostate downward and outward, in the line of the external incision. By following this precept, I cut the rectum in one of my first lateral operations, and I abandoned it ever after, cutting directly outward; now any precept that tends to endanger the rectum will also render the space under the prostate more liable to be laid open.

The importance of the subject must be my excuse for one further reference to anatomy. The hypoprostatic space, as above defined, is is divided diagonally from before to behind by that portion of the recto-vesical fascia, which, to avoid the confusion of names, I shall call the fascia interposita, from its lying between the rectum and bladder. It comes off from the deep perineal fascia behind the urethra, forms the proper tunic of the lower part of the prostate, and stretches from the base of that gland towards the rectum, in front of which it runs upwards, gradually assuming the characters of mere cellular tissue. When, therefore, the deep perineal fascia has been divided behind the urethra, there are two routes by which the infiltrated urine may reach the peritoneum; the one between the rectum and fascia interposita, which is circuitous and probably less fraught with danger; the other between the fascia interposita and bladder, which to lay it open requires a deeper wound, dividing either the fascia interposita or the substance of the prostate.

With respect to the best mode of extending the ordinary incisions of the Rectangular Operation for the purpose of extracting stones of large size, we must take for our guide the preceding considerations derived from anatomy, and the observed results of different modes of incision. We have seen that there is danger in extending the ordinary incisions laterally, and still more downward. In the former way we run the risk of subfascial infiltration of urine, and, in the latter, of the more dangerous form of infiltration into the subserous tissue of the peritoneum. The incision upward is, therefore, all that remains to us. It is clear that, if a moderate lateral incision has been first made, above the level of which the urine in the bladder can never rise, there will be no risk of urinary infiltration from an incision upward, even though carried beyond the base of the prostate, for the urine can never come into contact with such a wound. Even without the precaution of a previous lateral incision, Dr. John Thompson recommended the incision upward, and Dupuytren found it to be safe in practice.

The mode in which I perform the incision upward is by means of an instrument, which I name a *finger-director*, which I have for many years past used in cutting for anal fistula. It is a metallic cylinder, tapering from the rounded point to the base, open in front, and fitting like the finger of a glove to the index finger, while the handle attached to the back part of it lies flat upon the back of the hand; unlike the clumsy rectangular handle of the speculum ani, which, in imitation of the above instrument, has been more recently introduced to answer similar purposes. For these its structure shows that it was never intended; and those who know the history of the operation in question are aware that it was never till of late years generally so applied. This tapering cylinder, fitted upon the index finger, enters the wound with the same facility as the index itself; and the part of it, at which its further progress is arrested, gives an accurate measurement of the circumference of the wound of the prostate. The handle of the instrument being now securely held, while the open side of the cylinder is turned upward, the forefinger feels with much nicety the tense fibres stretched over the open groove which require to be divided; and this is done by one or more incisions with a curbed bistoury. The director can now be pushed further in, while an exact measurement of the size of the enlarged internal aperture is given as before. If need be, this incision can be carried beyond the base of the prostate, dividing the bladder to whatever extent may be deemed necessary; and this, with the lateral incision first made, will allow any stone to be extracted that can pass between the bones and the soft parts over them. After making a sufficient incision upward it is, I believe, never necessary to cut the right side of the prostate, and much safer not to do it. But a bilateral section of the external parts may be required for a large stone, and even a third incision directly upward, or a little to the left of the raphé of the perineum; thus giving the enlarged external incision a tricuspid form, not unlike a leech-bite on a great scale.

[The *Rectangular Operation* is now universally the one followed in Glasgow and the neighbouring parts of Scotland; but with the exception of Mr. Spence, Dr. Buchanan states that not a single surgeon in Edinburgh has ever performed it; and in other parts of the country it has been "opposed, misrepresented, and ignored." Dr. Buchanan says:]

Notwithstanding these unfavourable appearances, I am fully satisfied that the seed which I have committed to the ground is not destined to remain unproductive, but is even now in vigorous growth, and will ere long yield an ample harvest. The signs of this result are to me unmistakeable. Twelve years ago, when I first threw a pebble upon its placid surface, the opinion of all medical men in this country on the subject of lithotomy was tranquil and undisturbed. The lateral operation of Cheselden was accepted by all as an ultimate result of surgical ingenuity, and the boldest innovators for half-a-century before had never ventured farther than to modify the shape of the cutting instrument, or alter slightly the direction and the extent of the incisions. How different is the case now! Throughout all England the medical mind is in a ferment on the subject of lithotomy. Every periodical teems with observations and cases innumerable, all indicative of some great change which the public mind is in process of undergoing. And what is that change? It is exactly what I first advocated. All are now thoroughly determined, as determined as I then professed myself to be, to abandon for ever the old homicidal lateral operation for stone. Still further, they are all, without any exception, equally unanimous as to the direction in which the new movement is to be made. It is exactly the direction which I recommended to them. They all try to penetrate into the bladder, directly in the mesial or middle line of the perineum. To be sure, they do not do this exactly in the way in which I recommend that it should be done. But that would have been an honour too great for me to expect from metropolitan surgeons, more accustomed to give than to receive instruction. They prefer, not unnaturally, using their old instruments to my new ones. Some of them, however, have gone so far as to take my staff into their hands, and they express their surprise at the facility with which it slips into the bladder. But they do not call it by my name, or by the name which, as the inventor, I was entitled to give it. They prefer calling it, I am afraid less in compliment than in derision, "the Glasgow staff." Some have gone further, and have actually used the Glasgow staff in their operations. But do not suppose that they have followed my directions as to the mode of using it. They have more spirit. They hold the groove of the staff in the middle of the perineum, or right up against the pubes, as if to deviate as far as possible from the leading maxim of holding it under the level of the bulb of the urethra, which any tyro at Glasgow could prove to them to be indispensable. Last of all, there are surgeons more independent still. They do not use the Glasgow staff. They get a small bit added to either end of it; or better still, they drill a hole right through it, and then they consecrate the new staff by giving to it their own name, so that thenceforward no one can justly say of them that they use any other staff than their own. If, in fine, they wish to attain the highest distinction—to add the praise of ingenuousness to that of ingenuity—they publish a description of the new staff, while they admit candidly that some man, somewhere in Scotland, did previously devise and use a staff of a somewhat similar description.

The name of Rectangular Operation has been an unfortunate one, as tending to produce confusion in the public mind between the operation and the instrument used in performing it. It is impossible to dispel from the minds of many surgeons, more especially of those who have invented rectangular instruments and wish to use them in their own way, that "rectangular operation" can mean anything else than an operation performed with a staff of a rectangular shape, in whatever position that staff may be held. But to perform the rectangular operation, such as I have described it, the staff must be held in one invariable position, viz. with the groove under the level of the bulb of the urethra; and if the groove be placed in any other position, it is no longer the operation described by me that is performed. It is only when held in this position that the staff will permit the knife to penetrate into the bladder by the shortest possible route, and to cut outward, making a safe and sufficient opening for the extraction of the stone. Whoever operates with a rectangular staff held in the position ust described, performs essentially the operation which I recommend, whatever subordinate modification may be given to the staff. I owe, ndeed, and beg to express my thanks, to many ingenious gentlemen who have introduced such modifications, while they retain the essenial points of the rectangular shape of the staff, the true position of the groove, and the direction of the section outward. If I have not myself adopted any of the proposed modifications, it is because I have ot been able to persuade myself that the simple staff in the hands of man of ordinary dexterity, duly instructed in the way of using it, not a superior instrument to any of the more complicated staffs hich have yet been constructed, so far as I have seen them. As to hose gentlemen again who employ rectangular staffs, whether simple complicated, held in any other position than that described above, can only say that I cannot perceive the use which such instruments so held are intended to serve, and that personally I owe them no thanks, but the reverse, for the confusion which they have occasioned in the public mind as to the true nature of the operation which I have named "rectangular."

I must advert more particularly to two operations, in some respects very much akin to my own, which have obtained popularity in England. These are Mr. Allarton's median operation, and Mr. Hutchinson's operation with his rectangular catheter-staff. I may be allowed, without undervaluing the labours of those gentlemen, to urge the similarity and priority of my own.

Mr. Allarton's operation was first given to the public six years after mine—at a period therefore when the practical success of the latter had been fully ascertained. The object of both operations, in so far as incision is concerned, is the same—to penetrate into the bladder in the middle line of the perineum. I first applied the term *mesial*, in contradistinction to *lateral*, as a generic name for all such operations. Mr. Allarton, unwilling apparently to borrow anything from me, with much ingenuity changes two of the letters of this name, and calls his operation "the median operation." Now, as the one word is just as good as the other, and there is no difference in their signification, little objection could have been made to this change had he contented himself with calling his operation *a* median operation, but to call it *the* median operation, as if no other median operation were in existence, is to arrogate for it a degree of importance not warranted by its priority in point of time, or, as it appears to me at least, by its demonstrated excellence.

While mesial or median if it be preferred, is the generic name according to the nomenclature which I have proposed, the different modes of operating in the middle line are denoted by specific names indicating in what way the prostate is divided in each of them. If we apply this principle to Mr. Allarton's operation, it ought to be named the *medio-lacerant*, for I believe this term to express truly the mode in which for the most part, the prostate is actually divided ; but if any objection be made to that assumption, the operation might then be called the *medio-dilatory* which would express the double idea of the process of dilatation and the tediousness of that process.

To represent Mr. Allarton's section as a revival of the "sectio Mariana" is manifestly erroneous. Had Mr. Allarton imitated Marianus, he would not have cut in the middle line of the perineum at all, but to the left of it, and would have gone right through the corpus spongiosum and bulb into the canal of the urethra, leaving the membranous part untouched by the knife. Mr. Allarton's section is a much more scientific one, and the offspring of a much later age. So far as I know the history of lithotomy—if we pass over the crude attempts of the Celsian operators, and come down to the time when anatomy had become a science—Dupuytren is the first surgeon who ever attempted to reach the bladder by cutting from the middle line of the perineum through the membranous and prostatic portions of the urethra. To that great surgeon of the last generation—to whom I owe the respect and gratitude of a pupil to his teacher—I willingly professed my own obligations, assuming his bilateral operation as the starting-point of my own researches. But the bilateral operation was not the first mesial operation devised by Dupuytren. At an earlier period of his career he performed the very section now recommended by Mr. Allarton, but he abandoned it on finding that it was only fitted, even when accompanied with a free section of the prostate, for extracting stones of a very small size (très-peu volumineux). There can be no doubt that this is the source from which Mr. Allarton's operation has been actually derived, although he is not himself aware of it. His own account is, however, quite reconcileable with this view. He says he derived it indirectly from Bresciani di Borsa, through the medium of the 'Medico-Chirurgical Review.' But the Italian surgeon obviously adopted his section from Dupuytren's first mesial operation, just as he adopted the double lithotome from the second, in all cases where the size of the stone required it. I have thus shown that in object, in name, and in origin, Mr. Allarton's section is very similar to my own —the great difference being that Mr. Allarton operates with the old staff, as I originally did, to the great danger of the rectum and bulb of the urethra, while I operate with a staff especially devised to guard against these dangers.

There are only two steps in Mr. Allarton's section which he claims as altogether his own. These are keeping his finger in the rectum to guide his knife, and using the knife by stabbing as with a dagger instead of by cutting inward. Now these are the well-known steps of the rectangular operation. From these numerous coincidences, I am satisfied that Mr. Allarton has directly or indirectly profited by my labours more than if in the former way he has thought fit to acknowledge, or if in the latter than he may himself be aware of.

I hope I have said enough to show that Mr. Allarton has no title to the credit which is so generally accorded to him all over England of having introduced the mesial section for stone into this country. Mr. Allarton, as I have above stated, introduced his operation six years after mine, at a period when the practical success of the latter was no longer doubtful. Both operations are taken from Dupuytren—mine directly from his last and most perfect effort, which I have so modified as to render it, I believe, more safe and easy of execution, and more successful in its results: Mr. Allarton's, from an earlier operation which had been abandoned by Dupuytren himself on account of its imperfection, but which was revived by an Italian surgeon who wrote more than twenty years afterwards, and from whom Mr. Allarton indirectly takes it. The subject may be illustrated thus:—The city of Glasgow has just received a supply of pure water from Loch Katrine, the source of the River Forth; now if, six years after this, it were proposed to bring in another supply of water into the city, taking it not from the fountain-head, but much further down from the turbid stream, which meanders beneath Stirling Castle, the attempt would be quite parallel to Mr. Allarton's attempt to reintroduce into this country the mesial section for stone.

Although it is only with Mr. Allarton's section, or the first part of his operation, that I have properly anything to do, I cannot avoid offering a few remarks in passing upon the second part of it, in which unquestionably he shows himself a genuine disciple of Marianus. One great cause of the slow progress of the human intellect seems to be the tendency which it has, not to advance in a straight line, but to move onward in a kind of spiral curve, which brings it frequently back after a wide circuit to some point which it occupied long before. It is impossible to avoid this reflection when we see sensible men emulating each other in devising instruments, for the purpose of renewing, in the last half of the nineteenth century, the barbarities which were the disgrace of Surgery three hundred years before. It must be owned, however, that these proceedings are a genuine result of a principle which has long passed, and passes at the present day unquestioned as an axiom among lithotomists, viz., that the prostate glanp is more safely torn than cut. How a proposition so repugnant to common sense, and so inconsistent with our experience of every other tissue of the human body, should have met with a general acceptance with respect to tissues so dense and unyielding as the prostate and its capsule, is difficult to conceive. With whom it originated I do not know; but it has since passed as one of the arcana of lithotomy from one operator to another, and being screened from all general investigation, from the belief that none but lithotomists were capable of judging of it. I think, however, it most probably originated in the following way :—If a knife be pushed through an orange, or any similar solid, dividing it in the middle, but leaving it entire at the sides; and if thereafter the finger be forced through the opening, it will be found that a great deal of boring and apparent dilating or tearing is required to allow the finger to pass; but it is merely the effort necessary to change two plane and parallel surfaces into a cylindrical surface capable of adapting itself to the surface of the finger. Now, every operator, who thrusts his finger into the bladder, after cutting the prostate gland, experiences similar sensations, and is apt to imagine that he is dilating or tearing the substance of the prostate, when he is doing no more than adapting the two plane and parallel cut surfaces of the gland to the curvilinear surface of his own finger. Tearing the prostate is something very different; few tissues of the body resist it more powerfully, or would, I believe, be more injured by it. While the prostate ought never to be cut a single hairbreadth beyond what is necessary for the extraction of the stone, it ought, according to my view, to be cut, and not torn to the necessary extent.

Mr. Hutchinson, of London, has invented what he calls a rectangular catheter-staff, which he directs to be held in the middle of the perineum. What end can be proposed in holding such an instrument in such a position I cannot conceive, unless it be to render absolutely certain the division of the spongy body and bulb of the urethra, which are doubled over the angle of the staff, and cannot possibly escape the knife. I do not speak of what I have not seen. An eminent surgeon of this city took up at one time the same idea as Mr. Hutchinson, and insisted that his rectangular staff should be held up under the arch of the pubes. I held it myself, reluctantly indeed, but without one word of remonstrance, which I knew would have been vain. Such a gush of blood followed the introduction of the knife as I never before witnessed in lithotomy. It would have been called a flooding in the other sex; but most fortunately, although quite unlooked for by either of us, the bleeding stopped almost completely as soon as the patient was unbound and the staff withdrawn from the urethra.

With respect to the future of this operation, it might, perhaps, be most prudent to allow the future to speak for itself; but having once succeeded in foretelling its success, I am tempted to hazard another prediction. When the operation was yet a mere speculation in the dissecting-room, I had the presumption to anticipate, and say openly, that in ten years the lateral operation for stone would be a matter of history; and in less than half that time the new operation had completely superseded the old one on the only field where they have hitherto come into competition—in this city and around it. I now venture to predict that in less than the period above-mentioned, it will be the dominant operation throughout England, counting from the day when the operation shall be first performed in the theatre of a London hospital by a man who has carefully studied it, and is duly qualified to perform it. In saying this I am quite aware that while there are prophecies that bring about their own fulfilment, there are others which have an opposite tendency, and may even pervert the natural course of events by the opposition and hostility which they excite. But I know well of what I speak, and have calculated the chances upon both sides. The petty prejudices of individuals by which the operation is now opposed will become every day more feeble, and at length pass away; while what is conformable to truth and to nature is enduring, and will gather every day new strength from every fresh manifestation of its power.

I must, however, be understood to speak solely of England. To those who know Scotland it would not seem strange that a Glasgow invention should find little favour, and should not even be tolerated within the bounds of Mid-Lothian. The example of Mr. Spence, although his operation was successful, never met with a single imitator in Edinburgh; and what is excluded from Edinburgh is excluded virtually from a large part of the ancient kingdom of Scotland. No! I prophecy only of things possible. Dr. Livingstone—a pupil of the Glasgow school—will plant the new operation on the Zambesi and Shirwa; it will pass the famed Hydaspes, and penetrate into China and Japan—and perhaps, thereafter, a traveller from those remote regions will re-import it under a barbarous name as the invention of some Eastern sage, and then, but not till then, it will find a ready welcome, and become the dominant operation in the capital of Scotland.—*Med. Times and Gazette, April* 7 *and* 14, 1860, *pp.* 339, 364.

76.—ON MEDIAN LITHOTOMY.

By Thomas P. Teale, Esq., F.R.C.S., Surgeon to the Leeds General Infirmary.

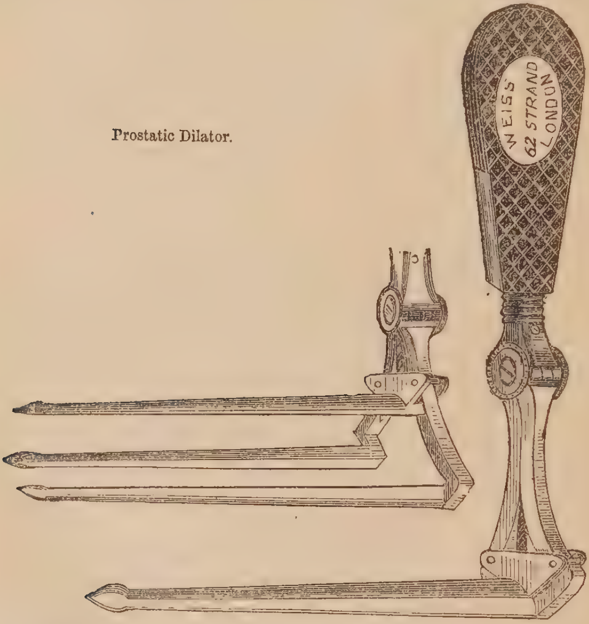
[Mr. Teale considers median lithotomy "not unlikely to supersede both lateral lithotomy and lithotrity." Mr. Teale observes, that difficulty is often experienced in children in consequence of the membranous portion of the urethra not being perfectly divided: he then continues:]

In children, the left forefinger of the operator is the best dilator, and generally the only one necessary. In adults, the question of dilatation demands more serious attention, as the finger alone will rarely be found sufficient, and some further mechanical aid will be required.

The instrument which I have until recently employed has been Weiss's three-branch dilator for the female urethra. The branches of this instrument, which expand by means of a screw, are slender, smooth, tapering somewhat towards their free extremity, at which part they measure, when closed, one-fourth of an inch in diameter, and at their base half an inch. They are four inches in length. With this instrument I have operated six times in adults; and in the majority of these cases found it to answer perfectly, but in some it appeared to be rather too slender and too short, although my object was attained by it. Anticipating the inconvenience that might arise from the use of this instrument in a subject extremely fat, or of large size, or with hypertrophied prostate, I consulted with Messrs. Weiss as to the best mode of remedying its defects. It was thought by us that two instruments, in extreme cases, should be used; one precisely similar to the old one, but with somewhat stronger blades, five inches in length, and a second instrument with broader blades to carry on the dilatation commenced by the first. These instruments were used, immediately after I received them, by Mr. Seaton, of Leeds, and I had the satisfaction of observing that they performed their duty perfectly. Shortly afterwards I used them at the Leeds Infirmary, in a case of unusual difficulty, which I must enter upon at some length.

The patient, aged forty-seven, was of very large size, and enormously loaded with fat. My finger (which, however, is not a very long one) when introduced into the rectum could not reach the apex of the prostate, but could only feel the staff in the urethra anterior to it. Evidently the bladder was very remote from the perineum, but I quite expected that the new instruments would be long enough to reach it easily. The incision was freely made. The long, narrow-bladed dilator was introduced; and, when it was pressed deeply, so that its base was imbedded in the incision, I felt the stone. After dilatation had proceeded sufficiently, the second dilator, with broader but somewhat shorter blades, was introduced within the branches of the former. which was then withdrawn. The blades of the second instrument were gradually expanded, so as to allow of the full-sized lithotomy forceps being passed within them. Contrary to my expectation, I was now unable to pass the forceps onwards into the bladder, on account of an unusual solid resistance to their progress. I imagined this to be owing to the back part of the prostate not having been sufficiently dilated, in consequence of the branches of the second dilator not having reached fairly through the prostate. The staff was again introduced, and the long, narrow-bladed dilator was passed along its groove, and pressed deeply inwards, and expanded. The parts now became sufficiently dilated to allow of the ready passage of the forceps into the bladder, and the stone—not without difficulty, on account of its remoteness from the surface—was seized; and was extracted through the prostate and external parts without any resistance. The stone was of lithic acid, and weighed six drachms. So deeply was it seated, that when it was grasped by the forceps, this instrument, although of the largest size, was buried in the wound to within a finger's breadth of the rings of its handles.

On the third day after the operation the patient died from inflammation of the cellular tissue of the pelvis. The examination after death, conducted by Mr. R. G. Hardwick, house-surgeon of the Infirmary, affords so instructive a lesson in reference to median lithotomy, that I cannot too strongly invite attention to it. The body of the patient was extremely fat, the abdominal layer of fat being two inches in thickness. The omentum and the sub-peritoneal cellular tissue of the abdomen and pelvis were in like manner extremely loaded with it, and a layer of fat one inch in thickness existed between the prostate and pubes. The bowels were distended with air; their peritoneal surface highly vascular, and there was a very slight quantity of lymph upon it. These appearances were more strongly marked towards the lower part of the abdomen. The cellular tissue of the pelvis, about the prostate and back part of the bladder, was evidently torn open, and was ecchymosed, softened, and much infiltrated with serum. The prostate was of natural size and structure. Its urethral surface exhibited, towards its vesical extremity, three wounds corresponding with the free extremities of the dilator, which, however, had not been long enough to reach fairly beyond the prostate. An instrument passed through the external wound into the bladder, showed that the distance from the perineum to the cavity of the bladder was five and a-half inches. The facts thus disclosed enable me to give the following explanation of the case. Owing to the extreme distance of the cavity of the bladder from the surface, namely, five inches and a-half. the long, narrow-bladed instrument, five inches in length, could only just reach the bladder when imbedded in the wound. As expansion proceeded, the instrument unavoidably receded to a small extent; the free ends of its blades thus became lodged within the prostate, and bore forcibly upon its mucous membrane. Three wounds were thus produced, which, however, in the aggregate, were not more extensive, and were no deeper than the incision made in lateral lithotomy. It further appears that the second and shorter dilator, introduced into the track of its predecessor, as soon as expansion was commenced, receded from the grasp of the prostate; and in its subsequent expansion, acted upon and tore up the cellular tissue between the prostate and rectum.



Prostatic Dilator.

On reflecting upon this case, I feel convinced that, if I had been so fortunate as not to be possessed of the second broad-bladed dilator, and had thus been compelled to continue the dilatation to its full extent with the longer narrow-bladed instrument, the operation, even in this unfavourable subject, would have been completed without material difficulty; and that the wounds of the prostate would not have subjected the patient to greater risk than the wound of this part inflicted by lateral lithotomy.

Availing myself of the information thus obtained, I have decided upon using, in future, one dilator only, and for this purpose have had another narrow-bladed instrument constructed by Messrs. Weiss, stronger and longer than the former one. The blades of this instrument are five and a-half inches in length, terminating in a slight bulb, which would check the tendency they might otherwise have to recede from the bladder. The dimensions of the instrument, of which the engraving gives a good representation, are the following: The blades five and a-half inches long, their conjoint diameter when closed, three-quarters of an inch at the base, and half-an-inch at the bulb.

The Act of Dilatation next requires careful attention, as upon its efficient performance the safety of the patient mainly depends. The dilator being introduced, the staff withdrawn, and the handle of the dilator turned up towards the abdomen, the surgeon, or an assistant, slowly, and at short intervals, expands the blades of the instrument. The prostate generally offers only moderate resistance, while the chief obstacle to expansion exists in the parts external to it, namely, the fibrous structures of the perineum. The chief pain in this part of the operation is caused by the stretching of these fibres. To facilitate, therefore, their yielding, and to diminish the pain, they should be gently touched with the edge of the knife, either inferiorly or laterally. When the dilatation of the parts external to the prostate has been carried far enough it will be found that the prostate itself has not been dilated quite to the same extent, on account of a slight yielding of the free extremities of the instrument; but it will generally have yielded enough to allow the large lithotomy-forceps to be introduced into the bladder, between the blades of the dilator, which may then be withdrawn. Should the stone be found to be very large, the blades of the forceps may be used for still further dilating the prostate. This is easily effected by withdrawing the forceps a little and then expanding its blades by repeated gentle efforts. The stone being seized, the process of extraction is effected by a moderate rotatory and tractile force, aided when necessary, by slight touches of the bistoury applied to resisting bands of fibrous structure. It is better thus to overcome the resistance than by violent traction.

In this operation, as in the lateral, it is important to use forceps of rather large size relatively to the subject, as the stone is more easily seized by a large than by a small forceps. The blades, being smooth and broad, greatly protect the parts transversed from the rough surface of the stone.

Concluding Remarks.—It is an interesting question, By what process the prostate, in median lithotomy, is widened so as to allow of the extraction of a stone through it? Is it by dilatation, or by laceration, or by both? At present, I presume, this question cannot be answered. Extensive observation, after death, of fatal cases can alone solve it. Experiments on the prostate in the dead body would prove little, as there is, I doubt not, a wide difference in the dilatability, and in the proneness to laceration, of the living and of the dead prostate. It is, however, probable that in the living body the process which suffices for the extraction of a stone of moderate size is one of simple dilatation; and that, in the case of a large stone, it is attended with some solution of continuity both of the mucous membrane and of the proper structure of the prostate. But, in this respect, it will only be analogous to that which must take place, from the forcible stretching of these parts, when a large stone is removed by lateral lithotomy; and moreover, it is unattended with the casualties incident to a deep thrust of the knife.

As to the relative fatality of the two operations, which alone can determine their respective merits, it is too early to speak. Prolonged and varied experience of the median is required. But, when the lateral operation in adults is known to be attended with a sad fatality, it is the clear duty of surgeons to try fairly other methods which afford a reasonable hope of better results. If the median should hereafter prove to be less fatal than the lateral, it must of necessity supersede it. If, on the contrary, the results of the median should be less favourable, it will become our bounden duty to relinquish the median operation.

As far as my own experience goes, the result of six recoveries in seven operations on the adult is favourable to the further testing of its merits. Difficulties may and will arise; but we must ascertain whether such difficulties are or are not insuperable.

It has been supposed that the median operation is applicable only to cases of small stones. My own impression is, that the largest stones, admitting of being extracted through the perineum, may be removed by the median quite as readily as by the lateral operation, provided that the resistance afforded by tense perineal fibres be relieved by gently touching them with the knife. In one of my cases, there were two stones, conjointly resembling in form an egg cut in two. These were seized and extracted as one stone, and were removed with perfect ease. They conjointly weighed 1½ ounce, and measured four and a quarter inches in their smallest circumference. The stone which I saw my son extract weighed 3⅛ ounces, and measured five and a-half inches in its smallest circumference. This stone was removed with about the same degree of extracting force as is usually required in the lateral mode.

It is stated by some surgeons that the operation is nearly bloodless. I have seen cases in which scarcely any blood was lost; but in most instances the bleeding at the time of operation was about the same as in lateral lithotomy. The subsequent draining, however, was much less. But, if bleeding of a more formidable character should occur after the median operation, we may feel assured that it does not proceed from large or from deep-seated vessels; and, moreover, the bleeding vessels may be rendered accessible to ligature or compression, by the aid of the three-branched dilator. In some cases I have thought that the stone was not seized so readily in the median as in the lateral operation; but on this point I would reserve an opinion until I have had further experience.

In conclusion, I will take a brief survey of the changes that have occurred in my own views and practice, as to the treatment of stone in adult males, since my first operation in 1826. These successive phases may be thus arranged :—

1. Lateral lithotomy in all cases.
2. Lateral lithotomy the rule, lithotrity the exception.
3. Lithotrity the rule, lateral lithotomy the exception.
4. Median lithotomy the rule, lithotrity the exception.

In the early part of my practice lateral lithotomy was alone thought of. As soon, however, as lithotrity received its last great improvement from the introduction of the screw-lithotrite, I adopted the operation; first in select cases where the stone was small: afterwards it was extended to stones of larger size, until it became, in my estimation, the operation to be generally preferred, leaving, however, a large number of exceptional cases, in which the stone was great, or the prostate much enlarged, or the bladder highly irritable, for lateral lithotomy. The next change was caused by the revival of median lithotomy in an improved form; and from the limited experience I have yet had of this operation, I feel a strong conviction that it will be the one which I shall generally adopt, leaving lithotrity to the few exceptional cases suitable for it, in which the patient has an insuperable objection to the knife.

It may be asked, Why was lateral lithotomy superseded in part by lithotrity? In reply, it may be said that the acknowledged fatality of the former operation induced surgeons to test the value of any other proposal which held out a better hope of success. In my own practice, thirty-five operations of lateral lithotomy in adults were attended with thirteen deaths, or one death in two and two-thirds of the cases. By the introduction of lithotrity, the result is improved by the addition of fifteen cases, with only one death, giving a result from lateral lithotomy and lithotrity conjointly, of fifty operations in adults, with fourteen deaths, or one death in three and a-half cases.

If the question were now asked, Why I would resort to any other operation when lithotrity has furnished me with a result of fifteen cases with only one death, I would reply, that in each case so treated, the patient has generally had to submit to several operations or "sittings," and that any one of these sittings might have been, as some of them were, followed by symptoms severe enough to cause serious anxiety.

In the hope of diminishing the risks to which, from the plurality of operations, the patients subjected to lithotrity are exposed, and of obtaining better results than lateral lithotomy affords, I have been desirous of giving a fair trial to the median operation.

I have now operated on adults seven times, three of these operations being performed on the same individual. The result of these has been one death in seven operations. If such a proportion of success can be maintained, it would be a great gain, as far as my own experience goes, upon lateral lithotomy, or even upon lateral lithotomy and lithotrity conjoined. I have great expectation that such will be the result; for, if we except the fatal case already described, presenting extraordinary difficulties, which might probably be overcome hereafter, the remaining six recovered so favourably that it was difficult to believe the patients had been subjected to a grave operation. They formed a marked contrast with the adult patients who had undergone lateral lithotomy, and who subsequently recovered.

I have no hesitation in extending the median operation to children, although lateral lithotomy in children has presented me a result of eighteen operations without a single death. Yet a considerable number of these patients were seriously ill, and caused me much greater anxiety than has been felt for the few young subjects whom I have seen under the median operation.

[Mr. Teale very properly brings forward the following fatal case, which occurred subsequently to writing the preceding paper. The patient was a gentleman fifty-two years of age.]

The perineal incision being made, the urethra opened, and the resisting bands formed by the deep fascia divided, the three-branched prostatic dilator, was passed along the groove of the staff into the bladder, which it entered without the least resistance. The staff was then withdrawn and the process of dilatation commenced. Immediately after dilatation was fairly begun, there was the usual gush of urine from the bladder. The process was then continued very gradually, to such an extent only as to admit the forceps—a straight pair—which passed readily into the bladder. On exploring the bladder very carefully with the forceps, I was unable, for a considerable length of time, to feel the stone, but at length perceived a rub against its surface high up towards the pubes. I was, however, unable, after several attempts, to succeed in getting it within the grasp of the instrument. The straight forceps was now withdrawn, and a curved pair introduced. With this instrument, the blades being directed upwards towards the pubes, the stone was struck, seized, and extracted. It weighed two drachms, and was of oval form, very much flattened. Scarcely any blood was lost. The patient on being placed in bed was free from pain, and appeared in a most favourable condition.

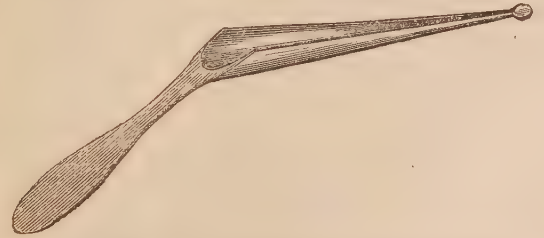
Two hours after the operation, before leaving by train, I visited him along with Dr. Wright and Mr. Hartley, and found him suffering from severe pain in the pelvis. The urine had not begun to flow. A few days afterwards I received a letter from Mr. Hartley informing me that the "pain" of which the patient complained at our visit on the afternoon of February 9th, "never left him; the urine came freely before ten o'clock in the evening, but there was no relief of pain. Peritonitis was evident next morning. He died at 10·15 on the evening of February 10th." No examination after death was obtained.

Remarks.—By this fatal case, the result of median operations in the adult performed by myself is two deaths after eight operations, and of such operations in the adult performed by Leeds surgeons, three deaths after sixteen. The very slight local or constitutional disturbance in my first six cases, led me to prefer median lithotomy to lithotrity in this instance. In my paper of December 10th it is stated, "In some cases I have thought the stone was not seized so readily in the median as in the lateral operation; but on this point I would reserve my opinion until I have had further experience." In the present case it was impracticable to seize the stone with the ordinary straight forceps. That there would have been greater facility of seizing it under the lateral operation, I do not presume to say; but the difficulty on this occasion was undoubtedly great. Probably the difficulty may have been increased by the peculiar form of the stone —on oval much flattened.—*Med. Times and Gazette, Dec.* 10, 1859, *and March* 10, 1860, *pp.* 573, 232.

77.—ON A PROBE-POINTED GORGET FOR MEDIAN LITHOTOMY IN CHILDREN.

By Thomas P. Teale, Esq., F.R.C.S., Surgeon to the Leeds General Infirmary.

[Mr. Teale describes this instrument in relating the following case. The patient who was nearly three years old, was admitted into the Leeds Infirmary on Dec. 10th.]



Probe-Gorget.—The proportion is exactly two-fifths of real size.

Before operating I requested Mr. T. Eagland, of Leeds, to make me a probe-gorget, as a guide for the finger into the prostatic urethra.

The blade of this instrument is three inches in length, and may be described as a hollow half-cone, three-fourths of an inch in diameter at its base, terminating at the apex in a bulbed probe half-an-inch long.

I operated on this child on December 17, and removed a stone the size of a chesnut, or, to speak more definitely, two and a-half inches in its smaller circumference, and three and a half in the larger.

The instrument easily glided along the groove of the staff so far as to allow the tip of my left forefinger to be lodged within the prostate. The finger was then pushed onwards while the guide was withdrawn. The tip of the finger readily touched the stone, which was seized and extracted with perfect ease, the operation being completed in little more than one minute. The child has not ailed anything since the operation, and at the end of a week is playing cheerfully in the ward.

As it is evident that the median operation will receive extensive trial, it is important to come to an early decision as to its details. In the hope of promoting this object, I venture to add another to the various instruments already submitted to the profession by Mr. Bowman, Mr. H. Thompson, and Mr. Armstrong Todd.

The instrument, which I have now described, has been found practically efficient in the removal of a large stone from a young subject. My colleagues and others who were present at the operation have kindly expressed their full approval of it. It thus appears that a probe-gorget has been employed in median lithotomy both by Mr. H. Thompson and myself, quite independently of each other, and with a satisfactory result in both instances.

In conclusion, I beg to offer the two following remarks:—

1st. That the "probe-gorget" here recommended, as well as the instruments recently described by Mr. Bowman and Mr. H. Thompson for use in children, should be regarded less as dilators than as guides for the finger, which in young subjects is the real dilator.

2nd. That the name of "prostatic dilator" is more strictly applicable to the instrument described in my paper of December 10, and I presume also to Mr. Armstrong Todd's instrument. [See Article 81 of this vol.]—*Med. Times and Gazette, Dec.* 31, 1859, *p.* 663.

78.—ON THE MEDIAN AND LATERAL OPERATIONS OF LITHOTOMY.

By John Erichsen, Esq., F.R.C.S., Surgeon to University College Hospital.

[It is a curious fact that the operation of median lithotomy was introduced some three or four hundred years ago, and continued to be practised up to the middle of the last century, when it fell into disuse upon the introduction of the "safer and simpler" plan of Cheselden.

Mr. Erichsen prefers the usual lateral operation to the median one now again brought into practice by Mr. Allarton. He says:]

I have found it advantageous to modify Mr. Allarton's operation in one or two points. The first, I think of some utility, is to use—which I did in the last case in which I cut by the median operation, about a week ago,—instead of the ordinary curved staff, a rectangular one, grooved from about one inch above the elbow nearly to the point, corresponding to the "Glasgow staff" introduced by Dr. Buchanan. Its use and advantages are, that, when in the bladder, the angle rests against the apex of the prostate and can be felt in the perineum, and you can judge of the exact point where to enter the knife (directing it so as to open the groove just below the angle), which you cannot do with the curved staff; the incision upwards also is limited, and there is, besides, less danger of wounding the rectum, the urethra being drawn upwards away from it, and not pushed down against it as with the ordinary staff. The knife should be straight backed, having the blade not more than two inches long; you can then tell to what depth you have entered it. I have found it advantageous in practice to carry a beaked director, shaped like a large hernia director, along the groove, after the incision has been made in the urethra, so as to open up the canal and thus to clear the passage for the finger. After having opened the urethra, I think it is better to dilate the prostate before withdrawing the staff; by pushing the finger slowly, with a rotatory movement, along its side, you get into the bladder with more ease and certainty; whereas, if you only use the probe, you may find it not stiff enough, and you are apt to push the bladder before you. It has been proposed to employ mechanical means of dilatation, instead of the finger, to open up the prostate and neck of the bladder; and I have had some dilators constructed by Mr. Coxeter on the principle of the two-bladed dilator of the female urethra. I have used them on the dead subject, as some of you have seen, but I have not ventured to use them on the living, lest, by the employment of screw-power, the same deep lacerations of the neck of the bladder should result that were so fatal in the old Marian operation. Indeed, I believe that their use would be fraught with danger from their liability to occasion rupture of the neck of the bladder, and if persisted in, I cannot but fear that they will bring discredit on the operation, reducing it to the condition of the old Marian, and repeating the dangers of that procedure. No safe dilatation can be effected except by the finger, with which no harm can be done, whilst it appears to me that the greatest possible mischief may be done with screw dilators.

In order to make a proper comparison between these two operations—the median and the lateral—we must take them seriatim. Now what are the dangers of the lateral operation? 1st, the difficulty in some cases of getting into the bladder; 2nd, hemorrhage; 3rd, the risk of wounding the bulb; 4th, of wounding the rectum; 5th, of too extensive an incision in the prostate, and opening up of the pelvic fascia; 6th, the difficulty in extracting the stone.

With respect to the general ease and simplicity of the operation, there is no doubt the surgeon will be more skilful in that operation which he has more often performed: so far as ease and simplicity are concerned, there is no great difference between them. In the lateral operation there is very seldom any difficulty in getting into the bladder, though surgeons have sometimes been foiled in this; but surgeons of the greatest skill have also had great difficulty in getting into the bladder in the median operation. The bladder tends to get pushed upwards and backwards before the finger, especially in children in whom the prostate is not developed; and unless the neck of the bladder be well opened, there appears to me to be a danger of tearing across the membranous part of the urethra, and of pushing the separated bladder on before you. So far as facility in entering the bladder then, is concerned, the two operations may be placed much in the same category.

As to the second danger—hemorrhage. In this respect the median has decidedly the advantage. If the incision be made in the middle line, without wounding the bulb, although there may be tolerably free bleeding at the time, yet there is no vessel that can furnish a dangerous consecutive hemorrhage; whilst in the lateral there are the dangers of arterial and of profuse venous hemorrhage, the knife coming into close relation with the artery of the bulb and others of some size. If the object were, therefore, simply to save blood, the median is so far better than the lateral. But after all, the danger of excessive hemorrhage in the lateral operation is but very small. With care, it will rarely happen that the patient loses a dangerous amount of blood.

Point third—wound of the bulb. This may occur in both, but is more difficult to avoid, and, indeed, very likely to happen, in the median, as the bulb sometimes so overlaps the membranous part of the urethra that it is difficult not to cut it; whilst in the lateral operation, by cutting low down, and entering the groove of the staff well back, and from below upwards, this may always be avoided. It is true that division of the bulb in the mesial line seldom gives rise to much hemorrhage, but cases have occurred to my knowledge, though not in my practice, in which patients have died from this cause after the perineal section, the blood regurgitating back into the bladder, and filling that viscus.

With respect to the fourth point of comparison. The rectum is not in much danger in the lateral operation, unless it be distended. In the median, on the other hand, the rectum is in considerable danger. If you perform this operation on the dead body, you will find the back of the bistoury very, I may say uncomfortably, close to the finger in the rectum; and if you place another finger in the wound, you will find them come into very close apposition just anterior to the prostate.

In the old Marian operation, the rectum used to be very frequently cut, air and fæces issuing from the wound.

Fifthly, as to the treatment of the prostate. I think this is very nearly the same in both operations. All are agreed that in the lateral operation but a limited incision should be made in the prostate and neck of the bladder, the opening being dilated with the finger, so as to avoid wounding the capsule of the prostate and the opening up of the pelvic fascia. The difference between an incision that opens the capsule of the prostate, and dilating this structure by the finger, is very great. The great object in lateral lithotomy is not to open up the pelvic fascia, and it is difficult, if not impossible, to tear this with the finger. Take an aponeurosis out of the body; you will find it very difficult to tear; but touch it ever so lightly with the knife, it separates at once. So, in the median operation, you may dilate the prostate to a considerable extent without opening its capsule. I have used the word "dilate," but dilatation is an erroneous term. If you look upon the prostate, which is one taken from a dead subject on which I have performed this operation, you will find it to be torn—not simply dilated, but lacerated. As you see in this preparation, there is an actual laceration of the substance of the prostate, but not extending into or through its capsule. A laceration of the substance of the prostate is of no consequence, and only becomes dangerous when it amounts to rupture of the capsule, when it exposes the patient to the fatal accident of extravasation of urine and diffuse inflammation of the pelvic fascia. Now, in the lateral operation, in running the knife down the groove of the staff, you may, certainly, unless care be taken, go beyond the limits of the prostate, and thus expose the patient to all these dangers. In the median, this cannot be done, if the knife be not used after the urethra is opened, the prostate being dilated solely with the finger. So far as this point, then, is concerned, I think that the median may be regarded as safer than the lateral operation, it being *impossible* to open up the pelvic fascia with the finger in the median, whilst they *may* be opened by the knife in the lateral.

Now for the last and most important part of the operation—the getting the stone out of the bladder. In lithotomy, your work is only half completed when you have entered the bladder; the most important part, that for which the operation is undertaken—the removal of the stone—has yet to be done. Here, I think, the lateral presents a decided and unquestionable superiority; so much so, that it must ever prevent the median becoming *the* operation for stone.

In performing the median operation you will find three points, or rather planes, of obstruction, between the surface and the interior of the bladder. The first is occasioned by the transverse muscles of the perineum, and, perhaps, also by the under portion of the deep perineal fascia. In the lateral operation you cut across this plane, and lay open the ischio-rectal fossa, giving abundance of room for the manipulation of the forceps and the extraction of the stone, along the base of the triangle formed by the rami of the ischium and pubic bones. But in the median you have to extract towards the summit of this space, at the apex of a narrow triangle, having the transverse muscles forming a tense bar along its base, and offering a material obstacle to the introduction of the forceps and the extraction of the stone. The second obstacle lies in the prostate; but as it is easily removable by dilatation, it cannot be considered a serious one.

The third, the deepest and most important, is situated at the neck of the bladder. In this preparation you find here a narrow, tense ring at the neck of the bladder beyond the prostate, and this bar remains intact in spite of the dilatation and laceration to which the prostate has been subjected. On introducing the finger, you will feel it grasped tightly by this ring. This inner ring of the neck of the bladder cannot be dilated beyond a certain point. I have found by experiments on the dead subject, that it cannot be expanded to a size more than sufficient to extract a calculus of one inch in diameter without laceration or incision. The existence of this ring is the greatest barrier to the extraction of the stone, and its laceration or rupture is well known as one of the most dangerous and fatal accidents in lithotomy. It is in consequence of the obstacle offered by this that the median operation is not available for the extraction of large calculi. Such a calculus as that you saw me remove by the lateral the other day, two inches in diameter, could not have been extracted by the median operation without the employment of a serious, probably fatal, degree of violence, because it could not be got out without laceration or rupture of the ring of the bladder. But it may be said, what is easier, when the finger is in the bladder, than to push a probe-pointed bistoury along it, and cut downwards and outwards, through these structures into the ischio-rectal fossa, if the stone be large? I answer, nothing could be easier or more simple; but what would be the consequence? Why you at once reduce the median to the conditions of the lateral operation. A free incision in the neck of the bladder and prostate increases the tendency to hemorrhage, opens up the fascia, and exposes the patient, in fact, to all the dangers of an ill-contrived lateral operation, destroying at once and altogether the principle of the median operation—viz., dilatation, and not incision; and if you do not gain space by incision, but attempt to extract a moderately large stone by dilatation of the parts, you will certainly not succeed, but your dilatation will end in a laceration, not only of the substance of the prostate, which is safe, but of the neck, and, perhaps, the base, of the bladder, which will be fatal.

In conclusion, then, it appears to me that the median operation, when performed in suitable cases, has the advantages over the lateral of being attended by less risk of hemorrhage, and with less danger of injury to the pelvic fascia; but that in consequence of the very small size of the opening that can be made in the bladder by it, *provided* *these advantages are maintained*, it is only applicable to certain classes of exceptional cases, and that it cannot be substituted for lateral lithotomy as the general operation for stone in cases not admitting of lithotrity.

My opinion as to the particular cases in which the median operation may with propriety be performed is this: In cases of foreign bodies, such as pieces of bougie, bits of tobacco-pipe, &c., being lodged in the bladder, the median is preferable, the body being small and easily extracted; so also for stones not exceeding one inch in diameter. But then, it may be said, calculi of this size can generally be safely subjected to lithotrity. That is true; but, in certain cases, lithotrity is not admissible; so that the median operation becomes limited to cases of small calculi in which lithotrity cannot be practised in consequence of irritability of the bladder, or of the patient being too weak to bear it. If a small calculus be lodged just behind the prostate, in a pouch which occasionally forms at the lower fundus of the bladder, you may come down on it at once by the median incision. In cases also in which lithotrity has been performed, and the patient is unable to expel the fragments, you may perform the median operation, and readily extract the detritus by the scoop, as it lies behind the prostate. But where you have to do with a stone of large size, the median is not, in my opinion, safe; such an amount of traction must be used as will infallibly bruise and lacerate the neck and base of the bladder, and expose the patient to infiltration of urine and deep pelvic inflammation—to all the dangers, in fact, of the old Marian operation; dangers which were so great that more than half the patients subjected to it perished, and which caused it to be abandoned for the lateral.

Finally, in cases in which the patient is so anæmic that the loss of an additional ounce or two of blood might turn the scale against him, the median is preferable to the lateral, as although it is by no means almost a bloodless operation, as is supposed by some, yet there is less hemorrhage during the performance, or rather, perhaps, less oozing after its completion, than in the lateral, and there is certainly not the danger of the profuse bleeding that is sometimes seen in that operation. It was for this reason that I performed the median in the last case that was under my care in private, and I am glad to say with success.

I have said nothing special about the performance of the median operation in children. Under the age of puberty, lateral lithotomy is a very successful operation; certainly the most successful of all the *great* operations in surgery. The median can scarcely be superior to it in this respect; and unless it can be shown to possess decided advantages over the lateral in ease of execution, it does not appear to me to be desirable to abandon an operation of proved safety for one that is still on its trial.—*Lancet, Dec.* 17, 1859, *p.* 607.

79.—ON MEDIAN LITHOTOMY.

By Wm. Bowman, Esq., F.R.C.S., Surgeon to King's College Hospital.

[Describing a prostatic dilator prepared for him by Mr. Weiss, Mr. Bowman observes :]

It consists simply of two blades, either united together at one extremity, or fitting one into the other there, and terminating in a slightly-curved probe-point, adapted to enter the bladder readily from the perineal wound along the groove of the staff. The blades being of thin steel, form a sufficiently rigid stem when closed, and admit of separation with flexion by the finger run in between them ; thus, of course, dilating the prostatic canal transversely without any forcing forwards of the neck of the bladder. The principle involved is the same as that of Mr. Holt's excellent stricture-dilator, which probably gave me the idea. The blades are of such a length as to extend from the external wound a little way within the prostate, and the probed extremity reaches from half an inch to an inch beyond. The blades, in diverging from the point of union, become gradually broader, and are a little hollowed on their opposed surfaces, so as to fit the finger. At their free ends they are curved outwards at an angle, and one of them is extended into a handle, which is to be firmly held by the right hand, while the left forefinger is being introduced as a dilator between the blades.

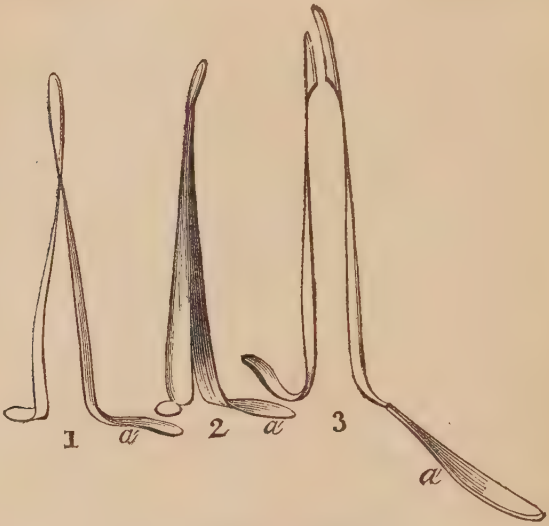
It is well known to lithotomists that in young children there is some risk of thrusting the neck of the bladder before the finger, as the latter seeks to enter its cavity, after the deep incision, and that operators deficient in coolness or dexterity have on rare occasions failed in consequence to enter the bladder at all, or have done serious, and even fatal, damage to its attachments. In such a difficulty, the dilator now devised might be of use. What is customary in such a case is to introduce the knife afresh along the staff and extend the incision in the prostatic region—a proceeding of some hazard in these young subjects, and probably much less safe than to dilate.

Recent opinions have more and more concurred in prescribing, with Brodie and Liston, a very limited incision of the prostate, and an extension of the opening by means of the finger, the blunt-gorget, or the blades of the forceps in withdrawing the stone. It is admitted on all hands that the prostatic canal is highly dilatable, or at least that it may be split open with little danger when partially divided by the knife ; and the safety of the modern lateral operation is acknowledged to depend mainly on the limited extent of the deep incision. Where a cutting instrument is run along the staff into the prostatic canal, it is in the nature of things that in the best hands it may sometimes wound more deeply than is intended, either by falling too much away from the groove of the staff, or by going too far into the bladder, and by having an inclination a little different from the intended one.

Hence, occasionally, cellular infiltrations or hemorrhages, adding heavily to the anxieties of the surgeon in the after treatment, and perhaps proving fatal.

I cannot but think that the median operation, aiming as it does only at an incision in the middle line, where no damage can be done, and carried only up to—not into—the prostate, must be highly worthy of adoption, unless experience should prove that it is attended by insuperable dangers more than counteracting its obvious advantages, and greater than those of the lateral operation. Dangers there undoubtedly are, as experience has proved, in attempts to dilate the neck of the bladder. I believe the dangers of slow dilatation are necessarily very great. modifying, as that process must do, the circulation of this important region, inducing inflammation in the neighbouring tissues, infiltrations, or venous contaminations. There may be dangers also in rapid dilatation as proposed by Mr. Allarton; but I confess it appears to me that the efforts of surgeons should be now directed to test these dangers, to avert them, as it seems quite possible to do, and thus to secure the removal of the stone with the least risk to life. There is no subject in surgery which requires to be considered from so many independent points of view as that of stone, for one case differs from another in the most important and essential particulars. A deep perineum, a large prostate, a bulky stone, no less than the varieties of age, are conditions materially altering the whole character of the operation; and I am inclined to think that further study of the method of dilatation following an incision of the urethra in the median line up to the prostate, will cause that mode of operating to gain ground in the estimation of surgeons.

To revert to the median operation of Mr. Allarton in young children, I lately found the same difficulty which Mr. Teale speaks of in carrying the finger along the staff or probe into the bladder after the incision of the membranous part of the urethra. In those subjects the prostate can hardly be felt by the finger in the rectum, and as the finger so placed runs nearly parallel to the membranous part of the urethra and neck of the bladder, it must be a little uncertain at what exact point the knife thrust in between the urethra and rectum enters the former channel. If it does not enter quite close to the prostate in cutting forward, so as to divide the lower wall of the urethra, it must leave a little of the membranous part of the canal undivided, as well as an important part of the deep fascia, and it becomes desirable, as Mr. Teale suggests, to carry the knife a little backwards up to the prostate in such cases. At least if this be not done, it will be found very difficult to insinuate the finger along the staff or probe, whether along its upper aspect or its side, so as to enter the bladder; for the end of even a small finger is too large and blunt an instrument to easily find the orifice of so small a canal as this is in young children. In these subjects also the ligaments and fasciæ holding the neck of the bladder in place are so slender as to yield and tear much more readily than in the youth or adult. Hence the idea of the dilator now described.



FIGS. 1 and 2.—Two views of the Dilator with united blades.
FIG. 3.—Form of a Dilator on larger scale with moveable blades.
aaa.—Handles to be held by the right hand, while the left forefinger moves up between the blades.
(The figures are on a reduced scale.)

There are two forms of it for trial. In the first, the blades are joined, and after the dilatation is complete, the finger would have to be withdrawn in order to disengage the instrument, though the bladder might probably be first explored by it, to ascertain the position and size of the stone. In the second form, one blade is received into the other near the probed extremity, and the blades thus admit of being withdrawn in succession by the right hand, while the left forefinger is still in the bladder.

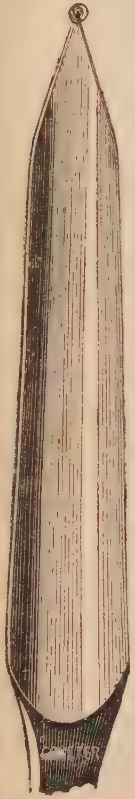
The whole length of the dilator for the youngest children is three-and-a-half inches; a larger scale might be adopted for young subjects of somewhat greater age.

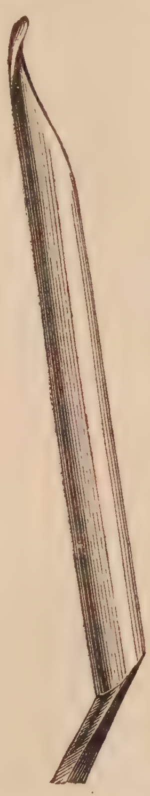
With regard to dilators for the median operation in adults, I have proposed to Mr. Weiss to make one on the same principle as the foregoing, but of such a size as to be opened, not by the finger, but by an ovoid plug, to be run in between the blades, of such a size as might be required in various cases according to the size of the stone. Without at all intending to prejudge the question, I think it worth trying whether an instrument on this principle would not offer some advantages over the three-bladed dilator as modified by Mr. Teale.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 614.

80.—ON MEDIAN LITHOTOMY.—NEW DIRECTOR.

By Henry Thompson, Esq., Surgeon to University College Hospital.

[The following case occurred to the writer at University College Hospital. He describes a new instrument used by him for the purpose of facilitating the proceeding.]





The patient was only three and a-half years old; and anticipating, therefore, that without some special means of carrying the finger along the staff from the membranous portion of the urethra into the bladder, considerable difficulty might be encountered, I had a director made for the purpose. The idea was suggested by the director which I have employed in circumstances somewhat similar—viz., for the purpose of facilitating the passage of a large catheter into the bladder after Syme's operation for stricture of the urethra, and figured in my work on 'Stricture,' second edition, p. 295. In the present case I added a probe point to run along the staff, and made the whole sufficiently wide to meet the necessity of the case—viz., about half-an-inch at the extreme breadth, but gradually tapering to the probe point aforesaid. The concavity is also considerable, to receive and direct the forefinger. The adjacent figure represents the instrument of the natural size. It now more resembles a gorget, but is taper throughout and thicker, so as to prevent the possibility of its edges cutting the tissues by which it passes.

Having made the incisions strictly in the manner recommended by Mr. Allarton, I introduced the director with the concavity upwards into the groove of the staff, and pushed it gently on into the bladder. I had now no difficulty in sliding the left forefinger along into the cavity and feeling the stone which was then extracted with very little difficulty. It was perfectly spherical and about the size of a marble. The child is doing admirably well. I should say that Mr. Bowman kindly lent me the instrument described in the preceding Article, and that I should have used it had not the other appeared to accomplish fully the desired object.

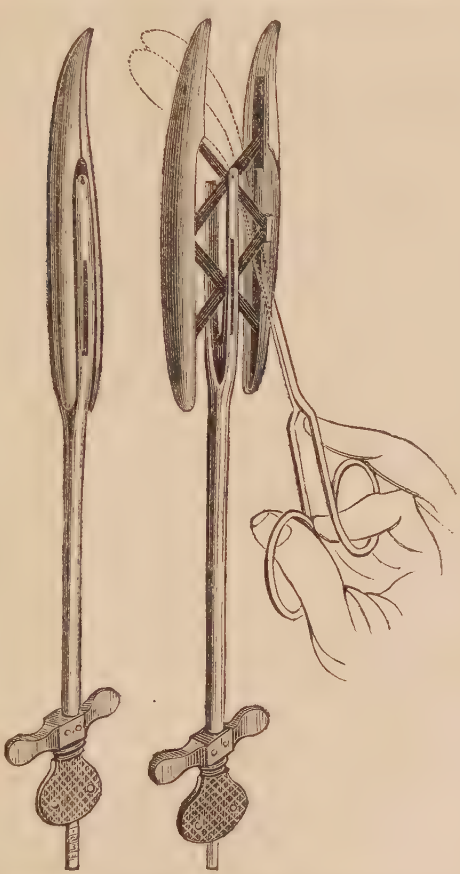
Perhaps I may be permitted to add, that when reviewing this subject at length in the 'Medical and Chirurgical Review' four years ago, in connexion with the works of Mr. Allarton and others on lithotomy and lithotrity, I expressed an opinion in favour of the median operation, and a belief that it was well adapted to many cases in which the lateral operation, and to some in which lithotrity, were then applied. All I have seen since that time tends to confirm that opinion, and the experience which Mr. Teale has so admirably expounded in his recent paper, must be considered as most important evidence in behalf of median lithotomy, and to warrant the increasing favour with which it appears to be regarded by many members of the profession.—*Med. Times and Gazette, Dec.* 24, 1859, *p.* 639.

81.—ON MEDIAN LITHOTOMY: A NEW PROSTATIC DILATOR.

By Armstrong Todd, Esq., Surgeon to the Marylebone Dispensary.

[The instrument recommended by Mr. Todd is on precisely the same principle as that used by him in strictures of the rectum.]

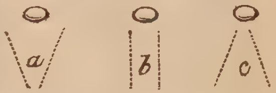
The blades, of course, are small and turned up at the extremity in in the form of a beak so as to run in the groove of the staff. Mr. Ferguson, of Giltspur-street, has made them of various sizes so as to suit the child or adult. For the mechanical description the reader is referred to the last volume of this 'Retrospect,' p. 144; the only difference which exists between the two instruments is the shape and size of the blades,—in fact its adaptation to the urethra, instead of the rectum, being made suitable for introduction through an artificial opening of small dimensions guided by a grooved staff. The principal benefit which I anticipate will be found from the use of this instrument is that the neck of the bladder and prostatic portion of the urethra are permanently kept dilated during the operation, permitting the introduction of the forceps and their free movement in the bladder, enabling the surgeon to search for and grasp the stone.



The parallel dilatation, also afforded by this instrument, I think, will be considered of great advantage.

In cases where the stone is so soft as to crumble under the forceps, it becomes a difficult matter in median lithotomy to wash out the bladder, owing to the power of recontraction possessed by the neck of the bladder and prostatic urethra. It is, therefore, here very necessary to have an instrument, which while it keeps the parts dilated, permits of the introduction of the syringe, and allows of free exit to the washed-out particles of stone. A case of this kind occurred to my friend Mr. Lloyd, not long ago, at St. Bartholomew's Hospital; and he found it was impossible to wash out the bladder owing to the cause above stated. The stone was so soft that but small portions could be removed at a time, appearing like mortar upon the blades of the forceps.

Mr. Bowman's instrument can hardly be called a dilator; it is evidently but a guide for the finger, which is in reality the dilator. Its great use, and perhaps its only use, is to obviate the danger of displacing the parts, and to prevent the production of intussuspection of the neck of the bladder, which might occur by using the naked finger. This form of instrument appears to me to have one great disadvantage—which is, that it dilates the superficial parts more than the neck of the bladder; in fact, the dilatation is of a conical form,—the base externally, and the apex internally, at the neck of the bladder, which obviously is the most dilatable portion of the canal through which we have to pass. Now it is evident that it is more difficult to draw a body such as a stone through a flexible conical canal, from apex towards the base, than from base towards the apex, as may be seen by the adjoining woodcut. Nay, more, the traction from apex to base in this case is likely to produce displacement and intussusception, quite as dangerous as, although in opposite direction to, that feared from the introduction of the finger without a guide. With the dilator which I propose, the movement is parallel, as I said before, but it could easily be made to dilate the neck of the bladder more than the superficial parts, so as to make the base of the cone internal, and the apex external, if such be deemed necessary.



The mode of using this instrument appears to me to be simple:—When the urethra is opened upon the staff, the dilator is passed along the groove, and the staff being withdrawn, it is properly adjusted, and the blades made to separate by means of the thumbscrew. When sufficient dilatation is obtained, the forceps are passed along between the blades, and dipped down in front of the crossbars into the bladder, moved about until the stone is felt and firmly grasped; the thumbscrew of the dilator should be then turned, so as to approximate the blades a little, and the instrument withdrawn, leaving the forceps and stone to follow in its track.—*Medical Times and Gazette, Dec.* 24, 1859, *p.* 640.

82.—CASE OF MEDIAN LITHOTOMY; REMOVAL OF THREE STONES, TWO OF THEM BEING ENCYSTED.

By R. Steel, Esq., M.R.C.S., Surgeon to the Blaenavon Ironworks.

[The case was in a little boy, aged nearly five years. From the symptoms the stone (which was readily detected) must have existed nearly two years.]

The patient being placed in the usual position for lithotomy, a staff grooved on its convexity was passed, and a straight bistoury was introduced, with its edge upwards, into the median line, about half an inch above the anus, so as to strike the groove in the staff; the knife was then carried upwards along the groove, dividing the membranous portion of the urethra, and withdrawn, cutting upwards through the integuments. The bottom of the wound was about three-quarters of an inch, and at the surface about an inch and a quarter, in length. A few fibres in front of the posterior layer of the deep perineal fascia were scratched through; a long ball-pointed wire was passed into the bladder, the ball being kept well pressed into the groove of the staff; the latter was then withdrawn, and the forefinger of the left hand, well greased, and guided by the wire, passed into the bladder by a gentle rotatory motion, without the least difficulty. On withdrawing the finger, a stone presented at the opening into the bladder, and was at once, and without difficulty, extracted by the forceps. It was of an ovoid shape, and measured one inch by three-quarters in diameter. I then explored the bladder with my finger, and on the anterior wall, high up in the pubic region, it came in contact with another stone; the only portion to be felt was a small glenoid cavity, about a quarter of an inch in diameter; the rest was firmly imbedded in a cyst. After two or three minutes' cautious manipulation with the forefinger (fortunately a long and flexible one), the stone was turned out of its bed, and fell to the fundus, and was at once removed; it measured three-quarters by half an inch in diameter. On again exploring with the forefinger, I found, to my intense annoyance, that its tip could just reach a third stone, in numerous foliated fragments, attached apparently by false membrane to the anterior wall, and well above the os pubis. The forefinger was not long enough to explore the stone, but with the middle finger I found that it was larger than the first one, that it was deeply imbedded in a kind of pouch, could not safely be got at by any instrument, and that it must be detached piecemeal by the finger. By dint of patience and great caution, this was effected. Seven or eight large fragments, about the same number of smaller pieces, a quantity of detritus and shreds of a kind of network of false membrane which had held the mass together, were, after dropping into the fundus, removed either by the forceps, the finger, or the injection of a large quantity of tepid water, patiently persevered with till I was satisfied not a fragment remained. This friable stone was found to consist of phosphate of lime.

I had never before performed Allarton's operation, and I felt misgivings that this complicated case might end fatally, and unjustly throw discredit on what I believe to be one of the most valuable improvements in modern surgery; I am, however, happy to say that the progress of the case has been most satisfactory, and may be summed up in a few words. About two hours after the operation the child seemed a little excited under the after-effects of the chloroform, but said he felt pretty well. That evening he said he was "quite mended;" but, although free from pain, he had a somewhat restless night; since which time he has gone on without any bad symptom, or any symptom whatever requiring treatment except a little cough, which he had been troubled with for some time previous to the operation. He never complained of pain but once, and that was during the passage of some urine. By the seventeenth day after the operation the wound had nearly healed; he had been passing urine freely through the urethra, for some days, but had done so a little from the first. He was very emaciated before the operation, but has been gaining flesh since; he eats heartily, sleeps well, and is delighted at being relieved from his terrible sufferings.

From this time the case ceased to require any attention; but the weather being very wet and boisterous, I would not allow him to travel home (a few miles off, and in an open conveyance) until the fifth week after the operation; at that time he had become fat, rosy, and full of spirits. I heard of him a day or two ago, and find that he continues perfectly well, and runs about as though he had never had anything the matter with him.

Remarks.—I feel I cannot speak too highly of Allarton's operation: the ease and certainty with which it can be performed must recommend it to every surgeon who has had opportunities of trying both it and the lateral; while the fewer tissues divided, and the insignificant loss of blood, must render it the safer operation. That it is suited to complicated cases the above must prove. The only part of the operation in which I expected difficulty was the dilating the opening in the bladder without dividing the prostate or even the posterior layer of the deep perineal fascia: here, however, I was wrong; the finger dilated the wound and passed into the bladder with the greatest ease, nor was there the least difficulty in grasping and withdrawing the first two stones when lying free in the bladder.

[A writer in the 'Lancet' signing himself "CHIRURGUS," observes, that we do not want a substitute for the knife in the cases of children, for hardly any operation can be more safe than the usual mode of lithotomy in these. What we want is a substitute for lateral lithotomy in the adult, or for lithotrity when peculiarities forbid the latter operation. He says:]

No amount of success in operating by the revived median operation on children will throw any light on the question whether it be suitable for adults where the stone *may* be large. There is nearly as much difference between the prostate of a child and that of an adult in respect to dilatability, as between plaster-of-Paris when it is yet in a soft plastic state and after it has set. Mr. Allarton, and the Italian surgeons mentioned in his book, confide in the finger as the dilator; and certainly, as respects simplicity, there is an advantage in this over the resuscitation, by Mr. Teale and others, of the blunt gorget; but still the question recurs, Will the finger be sufficient to dilate, and not lacerate, the adult's prostate? I believe, with Mr. Erichsen, that statistics have not proved the superiority, in late times, of the median over the lateral operation in adults; and until a better dilator than the finger or Weiss's instrument can be found, I should avoid this plan in all adult cases where the size of the stone is doubtful; justifying my avoidance by the fact that it has proved fatal in several recently reported cases of adults, and by the remembrance of the terrible fatality from the Marian operation recorded by John Bell and others.—*Lancet, Dec.* 24, 1859, *and Jan.* 14, 1860, *pp.* 641, 48.

83.—OBSERVATIONS ON LITHOTRITY.

By William Coulson, Esq., Surgeon to St. Mary's Hospital.

[After premising that lithotrity should *always* be preferred to lithotomy wherever the crushing operation is applicable, and that it is applicable in all cases in which the stone is small and the genito-urinary organs sound, the author proceeds to give the following general observations upon the operation of lithotrity.]

Like all other operations, lithotrity requires certain conditions which are essential to its successful performance. To understand what these conditions are, it is only necessary to recall to mind in what the operation of lithotrity consists. First, certain instruments are introduced into the bladder, which has previously received some fluid to protect it. Secondly, the calculus is seized and broken into small fragments. Thirdly, these fragments are passed from the bladder naturally, or are removed by the aid of injections and instruments. Lastly, it is an essential condition that these several steps of the operation should be completed without producing any injury to the urinary organs, or seriously disturbing the general health of the patient. To perform lithotrity in a scientific and successful manner, these different conditions must be strictly fulfilled; and in any case where it is probable that these conditions cannot be fulfilled, the operation of lithotrity is inapplicable.

It is evident, therefore, that in forming a judgment on the nature of any case previous to operating, two important points are to be regarded: First, the size of the stone to be removed; and next, the condition of the urinary organs. I do not dwell on the general health, because this usually depends on the state of the urinary organs, and is regulated by their condition. In every case, accurate information on these two points should be obtained before lithotrity is attempted, for neglect in this matter may be attended with serious consequences. This information can only be acquired by the most careful examination with those instruments and means which modern science has furnished.

Lithotrity is a peculiar operation. In nearly all other surgical procedures we see what we are about, or are assisted by the sensations of our fingers; whereas, lithotrity is carried on from the beginning to the end in the dark, the sensations which guide us being conveyed through the medium of instruments, and not directly by the touch. Hence it is of the utmost importance that as much information as possible should be obtained before the operation is commenced.

I have already shown that the principal difficulties of the operation are connected with the physical condition of the stone and the state of the urinary organs. Now, it is impossible to obtain precise information on these points without previous examination. In fact, this examination serves a double purpose: it enables us to determine whether lithotrity is suited to the case, and, next, it clears up many points on which the facility and ultimate success of the operation may depend.

The mode of conducting the first examination is as follows:—The patient being placed in the recumbent position, with the pelvis raised and the body depressed, four to six ounces of tepid water should be injected into the bladder. He should then be examined, either with a sound having the curved part short, or with a lithotrite, the curved portion of which is likewise short, while the blades are wide and flat. In ordinary cases the short-curved sound will be quite sufficient, but whenever there is difficulty in detecting the stone, or the co-existence of organic mischief is suspected, the lithotrite should be employed. The advantages of the lithotrite over the sound as an exploratory instrument have been clearly established. M. Civiale, in his fourth lecture on Lithotrity, mentions some cases illustrative of this point; and most surgeons of experience in calculous cases have succeeded in detecting with the lithotrite a small stone which had eluded their search with the sound. In some cases, however, even with the aid of the lithotrite, it is difficult to detect the calculus. The causes of the difficulty, are now, for the most part, well understood. The stone may be small and concealed behind an enlarged prostate, or the sensibility of the bladder may be so great as to render it extremely difficult to make the necessary examination. In other cases, the great capacity and feeble contractility of the bladder are obstacles to the discovery of a small stone. In ordinary instances, the calculus, when concealed behind an enlarged prostate, may be detected with the short-curved sound or the lithotrite, provided the instrument be first reversed, its handle then depressed and afterwards raised, so that the point of the instrument shall reach the bladder immediately behind the prostate. In these cases the instrument employed for the examination should be longer than those in ordinary use.

In some remarks on lithotritic instruments, in cases of enlarged prostate which I contributed to the 'Lancet' of Jan. 30th, 1858, I alluded to the changes in the genito-urinary organs produced by enlargement of the prostate, and requiring the use of peculiar lithotritic instruments. One constant effect of prostatic enlargement is elongation of the urethra, this elongation being mainly confined to the prostatic portion of the urethra. In these cases the orifice of the bladder is thrown backward in proportion to the development of the enlarged gland. Hence the surgeon is very likely to find the point of the instrument which he uses catch against the superior wall of the canal before it enters the bladder. The shorter the instrument he employs, the more likely it is that he will meet with this obstacle.

Another effect of prostatic enlargement. especially when confined to the middle lobe of the prostate, is to produce changes in the floor of the bladder. The extension of the middle lobe of the prostate backward forms a reservoir or sac behind the enlarged gland. In this saccular depression of the floor the urine dwells, and a calculus may be lodged, the detection of which is often very difficult.

In all these cases a longer lithotrite than that usually employed is indispensably necessary. The reasons may easily be made clear. The length of the canal is increased by at least an inch. The enlarged prostate encroaches another inch or more on the front part of the floor of the bladder. The calculus lies in a sort of pouch, before which the enlarged lobe of the gland rises like a bar. Hence the necessity for an instrument longer by two or three inches than that in ordinary use. I mentioned in the paper referred to, the case of a patient sent to me by Mr. James, surgeon. of Uxbridge, in whom I could not well reach the stone with ordinary instruments; and since that time, on a patient of Mr. Tatham, of Brighton, I was obliged to employ a yet longer instrument—one measuring fourteen inches. With this I could well reach the stone. With the long lithotrite it is not necessary to push the instrument up to the shoulder; but a short and sharply-curved beak must be used, and the pelvis must be raised. If the pelvis be not raised, the calculus may lie concealed behind the prostate. It is not sufficient merely to raise the lower extremities. But if the pelvis be raised, the calculus will be displaced towards the posterior wall of the bladder. In this way a cautious and practised hand will often fall upon the stone with remarkable ease.

If the stone cannot be felt by this method, we may succeed by examining the bladder in different states of its capacity. The best instrument for making this examination is the hollow lithoclaste of M. Civiale, from the use of which I have derived great assistance. This instrument is a small lithotrite, short in the blades, and having along the centre of the male branch a groove, through which any fluid in the bladder slowly escapes. The mode of using it is simple. Three or more ounces of tepid water are thrown into the bladder, and the lithoclaste is introduced before any of the fluid is allowed to escape. The instrument is now slightly opened, and while the fluid escapes through the central groove, the extremity of the opened instrument is slowly brought into contact with the different parts of the bladder. This method is likewise applicable to the final examination of the bladder after lithotrity, for the purpose of deciding whether any fragment of the stone remains behind.

Having recognised the presence of a stone in the bladder, and formed some opinion of its size, we also by this examination learn the degree of sensibility and the physical state of the urethra; we ascertain whether any considerable enlargement of the prostate exists, and discover the condition of the bladder; all which various conditions, it is unnecessary to observe, are those which influence the surgeon in the selection of lithotrity as the operation best suited to the case. When the urethra is in a healthy state, the prostate not much enlarged, and the bladder capable of retaining four or more ounces of fluid, the operation of lithotrity is easy of performance, and generally free from pain and danger.—*Lancet, Jan.* 14, 1860, *p.* 29.

84.—A NEW URETHROTOME FOR THE TREATMENT OF OBSTINATE STRICTURES OF THE URETHRA.

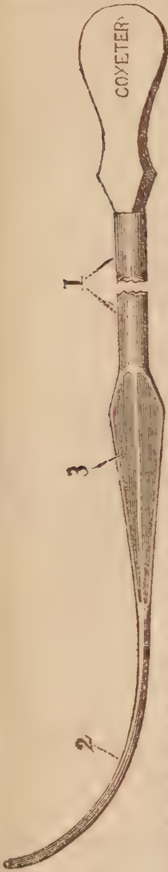
By John Marshall, Esq., F.R.S., Surgeon to University College Hospital.

[Mr. Marshall believes that the instrument described below, "involves a novel principle of action." Its construction is simple.]

It consists, as the subjoined wood-cut will show, of a solid steel bougie of the size of No. 10 in the stem, (marked 1,) of the size No. 2 for three inches at its curved end (2), and having an intermediate cutting portion (3) about one inch and a half in length.

In the construction of this part of the instrument its peculiarity consists. It is here flattened on its upper and under surfaces, so as to form a thin wedge, with two lateral slanting edges, and having a circumference gradually increasing from No. 2 to rather more than No. 10 size. Its upper and under surfaces are neither plane nor convex, but present a central rib, with two lateral hollows, so as to allow of but little hold upon these surfaces when the instrument is in use, and yet maintain its stiffness and its general wedge-like form. At the same time, the grooves also serve to contain some unctuous substance by which to lubricate it.

The lateral borders of this part of the instrument are ground to a smooth, stiff, blunt edge,—something like that of a sword,—not sharp enough to cut the finger or the lip, or even the tongue, when pretty firmly drawn over those parts, but readily severing any yielding though tough organic structure, such as leather, cat-gut, or even string, drawn over it with a sufficiently firm, sliding movement.



In using this instrument, its smaller end is passed through the stricture to be divided; and if this be carefully done, provided the orifice of the urethra has been previously dilated, if unusually narrow, no sort of cutting or even scratching of the urethra in front of the stricture takes place. The point of the instrument having been ascertained to be in the bladder by the freedom with which it can be moved about, the operator, then standing on the right side of the patient, (who is supposed to be recumbent,) and holding the handle of the instrument in his right hand, and the penis well drawn forward with his left hand, is enabled, as I have found in practice, to pass in the instrument through the stricture, in a few seconds, with a surprisingly small amount of force. The instrument acts smoothly, without catch or jerk, and with a sensation to the operator as if it were penetrating something as soft as cheese.

The operation can, of course, be performed with or without chloroform, but to a patient who had no chloroform the slight pain caused by cutting was less inconvenient than that produced by previous attempts at dilatation. In the case alluded to, the man—a retired Indian soldier, fifty-four years of age, and a patient in University College Hospital—had suffered many years from a narrow organic stricture, accompanied with albuminuria. With some patience, after several weeks, the stricture was dilated so as just to admit No. 3. In this condition, on the 24th of February, it was divided by the operation above described, when only a few drops of blood escaped from the urethra. A No. 9 elastic catheter was then passed with the greatest ease. Six hours later, the catheter was removed. From that time the patient has passed his urine in a full stream. On the day after the operation, a No. 10, being tried, did not pass, owing, probably, to swelling of the sides of the canal; but on the seventh day, Nos. 8, 9, and 10, on the eleventh day, Nos. 9, 10, 11, and on the fifteenth day, Nos. 10 and 11, slipped into the bladder at once, and were withdrawn without any grasping by the walls of the urethra. I may add that neither shivering nor any other constitutional disturbance followed the operation, and that the health of the patient, as might be anticipated, has improved.

The instrument now devised is evidently not a mere wedge-shaped dilator, and, I may remark, is wholly inefficient if its edges are left rounded or non-incisive. Neither does it act in the same manner as the fine-bladed sharp-edged urethrotomes now employed, whether these cut from behind forwards or from before backwards; for whilst, beginning from the internal surface of the strictured part, it divides all the hard, resisting structure, it will merely stretch any tissue beyond, which is soft and elastic enough to yield. At all events, it would seem far less likely, when deliberately used, to cut through the fibrous tissue of the corpus spongiosum urethræ than the thin sharp blades of existing urethrotomes. The very small amount of hemorrhage following its employment favours such an opinion; and if the fact really turn out to be so, there will necessarily be less risk of urinary infiltration and abscess.

Whilst, however, the nature and extent of the incisions made by the instrument, and the permanency of relief afforded by it, must be the subjects of future enquiry and experience, I may be permitted to direct attention in the present communication to its simplicity of construction, the facility and safety with which it may be used, and the difference between it and the ordinary urethrotomes.

This new urethrotome may, of course, be made of different sizes; but a smaller size at the point than No. 2 might be less safe for ordinary purposes, and a smaller stem than No. 10 is not desirable, unless for a traumatic stricture in a boy's urethra, or in a case in which that canal is unusually narrow. I may further state that I have had a *straight* one made for the division of strictures in the fore part of the urethra, and also a curved *hollow* one, which acts as a catheter, and thus will determine in cases of peculiar difficulty the fact of the end of the instrument being really in the bladder, and will enable that organ to be injected with water, so as to remove its anterior wall away from the point of the instrument, and subsequently furnish evidence of the elastic catheter having reached its destination. I have also had the instrument made with the cutting edges both above and below, instead of at the sides; but for reasons which I need not now state, I have adopted the latter construction.

Finally, it is obvious that a like principle may be applied to instruments for dividing indurated strictures in other canals. It is also evident that it may be adapted to blades capable of being concealed or projected, or to a blade sliding over a smaller guide which has been previously passed.—*Lancet, April* 14, 1860, *p.* 370.

85.—CASES OF STRICTURE OF THE URETHRA TREATED BY "SLITTING UP."

(Under the care of W. A. Hillman, Esq., Surgeon to the Westminster Hospital.)

[The plan pursued by Mr. Hillman in these cases was first introduced into the Westminster Hospital by Mr. Holt.]

The method consists in dilating the stricture, if necessary, until it is sufficiently relaxed to allow of the passage of an instrument about the size of a No. 3 catheter. The patient is then placed under the influence of chloroform, and the "stricture-dilator" is introduced. This instrument resembles somewhat a catheter, only slightly curved at the end, and split down the middle, so as to consist of two longitudinal half-cylinders, which admit of being separated to some extent, and are fixed on a central stem or axis. When the instrument is fairly in the bladder, the spring or collar which connects the blades is relaxed, and a large perforated rod, about the size of Nos. 10-12 catheter, bevelled off at the end. is fixed on the central axis, and forcibly driven down the urethra. The effect of this is to suddenly dilate, and in most cases to rupture, the tissues of which the stricture is composed; as is evidenced by the escape of a few drops of blood. The dilator is now withdrawn, and a No. 10 or 12 catheter (according to the size of the urethra) is introduced, and the water drawn off. This catheter is taken out, and the patient sent to bed, and generally a stimulant given. He is left quiet during the following day; and, on the second day from the operation, this catheter is reintroduced, and generally passes readily. Sometimes the stricture, or the urethra, may have contracted again to the size of No. 8. This plan is extensively followed by Mr. Holt, Mr. Hillman, and Mr. Power; and a very great number of cases altogether have now been treated in this manner, the greater number having been under Mr. Holt's care. No bad symptoms have followed in any case; and we are informed that the stricture shows no greater tendency to recontract than is noticed after any other plan, and that several persons have been seen in whom the cure had already lasted three years.

We need hardly dwell on the advantages of a plan which, if successful, saves so much time and suffering as is involved in the treatment of a severe stricture by the ordinary method of dilatation; and if the forcible rupture of a stricture should prove as safe a proceeding in the hands of others as it has done in those of the surgeons of this hospital, we do not doubt that it will soon come into more general use.

Case 1.—T. W. has suffered from urethral stricture for about fourteen years, the symptoms having gradually become more and more severe. He sometimes passed water in a very small stream, but more frequently only in drops; in his own words, he "was always making water". There was considerable admixture of mucus and pus with the urine, and he suffered from considerable prolapse of the rectum.

On his admission, a No. 1 catheter could not be passed into the bladder; but after a short stay in the hospital, a No. 1, and subsequently a No. 3, silver catheter was passed with some difficulty. After a short residence in the hospital, whilst he was under the influence of chloroform, Mr. Holt's "stricture dilator" was introduced, and the stricture split up. The dilator having been withdrawn, a No. 10 silver catheter was passed into the bladder, the bladder emptied, and the catheter removed, a few drops only of blood being lost. He had some smarting pain, as the urine passed along the urethra, for two or three days, but no unfavourable symptom of any kind followed the operation.

He was shortly discharged from the hospital, passing his urine in a good stream, without pain, and was not obliged to empty the bladder at night, as he could retain more than a pint of urine without inconvenience, the mucus and pus having disappeared, and the prolapse of the rectum nearly subsided.

Case 2.—P. F., aged 56, was admitted under Mr. Hillman's care, on September 13th. He had suffered from stricture of the urethra for at least sixteen or eighteen years. The urine passed in an exceedingly narrow stream, almost indeed drop by drop; and the bladder was so contracted and irritable that it would retain scarcely any water. The patient was in the hospital for four weeks before any instrument could be introduced into the bladder; after five attempts, however, a silver catheter, smaller than No. 1, was passed, and subsequently a No. 3.

October 18th. The "stricture dilator" having been introduced, and the patient put under the influence of chloroform, the stricture was split up, a No. 10 silver catheter was introduced; the bladder was emptied, and the catheter immediately withdrawn. A few drops only of blood escaped from the urethra. He was made warm in bed; had some hot gin and water administered. No bad symptom followed.

He left the hospital much improved in general health, passing his urine in a good stream; the bladder having so far recovered its healthy condition, that there was no necessity for him to pass urine during the night.—*British Med. Journal, Nov.* 19, 1859, *p.* 933.

86.—THE IRON-WIRE SETON IN HYDROCELE.—CASES.

By Thomas Davidson, Esq., L.R.C.P., L.R.C.S.E., and T. Sympson, Esq., M.R.C.S.E., Surgeon to the Lincoln County Hospital.

[Mr. Davidson's case was in a man aged 67, the subject of hydrocele of the right testis for ten years. He had been repeatedly tapped; the largest amount reported is thirty-three ounces.]

In August last, the hydrocele being of such a magnitude that it prevented the man from following his usual employment, that of a carpenter, I resolved to give him the chance of a permanent cure by employing the wire seton. On the 22nd of August I introduced six threads of the proper wire from below upwards, with a curved needle suitable for the purpose. The fluid, which would measure at the least forty ounces, speedily escaped. I allowed the seton to remain thirty-six hours, when I removed it gently. At this time, everything seemed favourable, there being no pain or uneasiness; but in the course of twenty-four hours intense inflammatory action was lighted up in the tunica vaginalis and testis, and distending by effusion the scrotum to an immense extent. It was only by the most assiduous attention, and after using every available means for reducing the inflammation, that its course was ultimately checked. On perceiving fluctuation, I made a free incision into the centre of the tumour, and gave exit to a considerable quantity of fetid pus. In about ten days all discharge ceased and the wound healed; suppuration, however, repeatedly recurred, obliging me from time to time to open the abscesses. The patient was now in the most critical situation, being reduced to a state of great debility, and but for a liberal supply of port wine, beef-tea, &c., the man must have sunk.

The discharge still continuing fetid, I injected into the cavity a solution of chloride of lime morning and evening, keeping the wounds open for many weeks together, and by this means I fortunately succeeded at last in effecting a cure.

By the 10th of November, the wounds were healed. all inflammation had ceased, and the testicle was reduced to its natural size, so that the hydrocele might be pronounced perfectly cured.

The man has not up to the present date regained his strength sufficiently to enable him to resume work, although I expect that in a week or two he will be able to do so. From the extent of the suppuration, and constitutional suffering which occurred in this case, I am apprehensive that without some modification of the operation, the employment of this method of cure will be rather unsafe as a general practice, as we see from the case published in the 'Medical Times and Gazette', for December 10, by Mr. J. B. Thompson, and from another reported by Dr. Gillespie some months ago, that the same constitutional disturbance and excessive suppuration takes place in the young and robust subject, as well as in those more advanced in life.

[In a case of hydrocele similarly treated by Mr. Sympson, the patient was a child of four years old, and the hydrocele the size of a hen's egg. It had been twice tapped with a fine trocar. He was admitted into the Lincoln County Hospital on March 14th, 1859.]

On the 15th, by means of a hemorrhoidal needle, a single silver wire was passed through the sac, and secured by twisting together the two cut extremities. This was allowed to remain in twenty-four hours, by which time the scrotum had become greatly tumefied, and very tender, red, and painful. The constitutional symptoms ran high, and on the 18th, it being plain that suppuration had occurred, an incision was made, which gave exit to about an ounce of pus. After this another abscess formed in the lower part of the scrotum, which required opening on the 25th. Pressure, by means of strapping, was applied on April 10, and on May 16 he was discharged with a scarred and roughened scrotum, but without trace of the hydrocele.

Remarks.—At first I was inclined to ascribe the ill effects produced by the wire in this case to its having been left in too long; but I now think them more likely to have arisen from some peculiarity of constitution: such results occasionally occurring after other methods of treatment. To all appearance the boy was in excellent health when he entered the hospital, and there seemed no reason why so simple a procedure as the passing of a silver wire through the tunica vaginalis should occasion so large an amount of irritation; but, probably, had any other operation been substituted, it might have been attended with a similar result.

As the treatment of hydrocele by metallic seton is still *sub judice*, I have thought it right to place this case on record.—*Med. Times and Gazette, Dec.* 31, 1859, *pp.* 652, 653.

87.—FRACTURE OF THE PELVIS AND RUPTURE OF THE URETHRA.

(Case under the care of James Paget, Esq., F.R.S., St. Bartholomew's Hospital.)

The following is an example of the most difficult class of cases of urethral stricture with which the surgeon can ever be called to deal. When a stricture is simply impassable, there is hope that by patience it may be dilated; but when, in addition to that lesion, there exists a false passage leading past the stricture, there is little prospect that the instrument can ever be made to find its way into the proper channel. If the false passage be large, and if it lead merely out of one portion of the urethra into another, or into the bladder, it is quite possible that the patient may not suffer greatly, and that no surgical interference may be called for. But if, on the contrary, a false chamber has been formed, admitting of an accumulation of urine, and subjecting the patient to the risk of urinary abscesses, infiltration, &c., the case becomes at once one of extreme difficulty and danger. In the case before us a severe injury to the urethra and pelvic contents had been received, and, as a consequence of extravasation, a large subsidiary cyst had been formed between the pubes and the true bladder. Into this false bladder the urine constantly escaped, and although catheters were easily introduced to their full length, yet they passed into it, and not into the proper receptacle for that fluid.

It is in cases such as this that the operation of perineal section is at once peculiarly valuable and peculiarly difficult. In commencing its performance the surgeon can never feel sure that he will be able to find the proximal or vesical tract of the urethra; and if he should fail in this, the end of the procedure is not attained. The rules for success are to cut freely, and to keep most carefully in the median line, and to obtain by examination per rectum a clear idea as to the position of the prostate.

[The patient was a miner who, two years ago, had received a severe blow on the pelvis, by the falling of a piece of rock. This was followed by paralysis of both legs, and retention of urine. A catheter was passed, and a large quantity of pure blood drawn off. This was repeated for three days with the same result. On the fourth day some urine flowed. The perineum became hard and tender, and was incised on the fifth day, and a fistulous opening remained. He continued twelve weeks in the Chester Infirmary, the catheter still requiring to be passed; the urine flowing still through the fistulous opening left, at the spot of incision. He was under surgical treatment the whole of the two years before admission under Mr. Paget when he had entirely recovered from his paralytic symptoms, but still continued unable to relieve his bladder.]

On admission Mr. Paget was able to pass a catheter (No. 5 or 6). He remarked that the instrument went in "with all the facility of error," and stated that he felt sure that it did not enter the true bladder. Resistance was encountered at a certain point. Urine was drawn off, but its flow was preceded by a discharge of pus. Having consulted with his colleagues, on December 5 Mr. Paget determined to perform perineal section. The patient was placed under the influence of chloroform; a grooved staff was introduced, and an incision next made in the middle line. A hard, cartilaginous mass was encountered, which Mr. Paget freely divided. An attempt was now made to discover the vesical tract of the urethra by passing a large probe from the perineal incision. This was successful, and a catheter having replaced the probe, the urine flowed freely, thus proving that the cavity of the true bladder had been reached, since the fluid contained in the false sac had been drawn off in the beginning of the operation. Mr. Paget next passed a No. 8 catheter through the penis, and having brought it out at the wound, next guided it on into the bladder, by the side of that previously introduced. This was left in, no hemorrhage, shivering, or any other unfavourable symptoms occurred. The bowels were left undisturbed several days, and during a whole week the catheter was left in, causing no uneasiness. He continued to improve in every respect until November 23rd, when he had an attack of severe pain in the testis and loin. This was relieved by warm baths and a narcotic, and did not return.

Dec. 3. He was now able to pass the catheter himself. It was left in an hour daily. A little urine still passed through the wound.

20th. It is noted,—"He is going on very satisfactorily. He passes the catheter every day, and leaves it in an hour. The urine now comes the right way. Mr. Paget desires him to pass the instrument for a month longer."—*Med. Times and Gazette, Feb.* 11, 1860, *p.* 142.

88.—*Varicocele—Ligature and Subcutaneous Section.* (Case under the care of Wm. Bowman, Esq., F.R.S.)—[This case occurred in a young man, the varicocele having existed six months, and prevented his following any employment, from the pain in his testes, loins, &c.]

Mr. Bowman stated that the operation was based on exactly the same principle as that of subcutaneous ligature of veins for varicose ulcers. He passed two needles, an inch apart, under the veins, carefully keeping the vas deferens with the artery away, and then applied the ligature over them, just tight enough to occlude the cavity of the veins, but not to cut them. The veins were then divided between the needles by a tenotomy knife, the wound made being very small. A pad was next placed over the incision. Mr. Bowman stated that he had treated varicocele by this method on several occasions with very satisfactory results. There was no hemorrhrge, nor did suppuration follow, and thus there was no chance of pus, and also none of decomposed blood being absorbed into the system. He should remove the lower needle probably on Monday, and the upper one on Wednesday. The neuralgia would probably remain for a little time after the operation. He stated that atrophy of the testis, so frequent in varicocele is due to impaired nutrition from obstructive circulation, the same condition precisely existing in varicose ulcer, and both also remediable by the same means, viz., obliteration of the varicose veins.

[November 30th. To-day Mr. Bowman removed the second needle, the lower one having been removed on Monday. The man is doing well. There is no suppuration.]—*Med. Times and Gazette, Dec.* 3, 1859, *p.* 552.

DISEASES OF THE SKIN.

89.—ON THE TREATMENT OF FAVUS.

[Despite the much-varied forms of treatment the disease often remains very obstinate. Porrigo is very often mistaken for favus, and consequently many cases supposed to be "cured" are not cases of favus at all. The following plans of treatment are those of different modern dermatologists, well acquainted with the diagnosis and true pathology of the disease.]

Mr. Startin's plan of treatment, as adopted at the Hospital for Diseases of the Skin, is to clean away the scabs, either by poultices or careful washing, and then apply freely an ointment containing sulphur and the ammonio-chloride of mercury. The latter should be in proportion of about a scruple to the ounce of lard. By this plan we have often seen scalps which have been covered with favus got perfectly clean, and apparently cured in the course of a few weeks.

Dr. Jenner's treatment is, like Mr. Startin's, based on the principle of employing parasiticidal remedies, and consists in the free use of lotions or baths containing sulphurous acid.

Dr. Hughes Bennett endeavours to prevent vegetable growth by the

the exclusion of atmospheric air, applying with that object cod-liver oil, and covering the scalp with oil-silk.

Dr. Neligan employed an ointment of the iodide of lead, after having cleaned the scalp by poultices and alkaline washes.

We cannot think that it is a matter of any very great consequence in what manner the cryptogam-destroyer is used, or whether the drug chosen be creosote, mercury, or sulphur. Whatever plan is adopted, the secret lies in following it out perseveringly, and without suspending it, because the disease may appear for a time to be well. The removal of the hairs from the affected part by means of tweezers, is a measure which, if the patient will allow it, and the surgeon has zeal enough to adopt it, is one likely to materially increase the chance of genuine cures being obtained.—*Med. Times and Gazette, Dec.* 31, 1859, *p.* 655.

90.—*Eczema of the Hands.*—M. N. GUILLOT, of the Necker, employs the following ointment with success in the treatment of eczema of the hands:—Lard 30 parts, subcarbonate of soda, oil of cade, tar, of each from 2 to 4 parts.—*Med. Times and Gazette, March* 24, 1860, *p.* 299.

91.—*On the Treatment of Squamous Diseases.* By WILLIAM MOORE, M.B., M.R.I.A., Dublin.—[After giving in detail numerous interesting cases of squamous disease of the skin, illustrating the different forms or varieties of the class, Dr. Moore makes the following remarks on the treatment:]

As a rule this genus is met with in a sub-acute form, and frequently accompanied with hepatic congestion; consequently, in the first instance, it is advisable to exhibit a mercurial or other chologague till the functions of the liver are restored. Then the proper line of treatment is tonic and alterative, with local appliances which may tend to induce increased dermoid action, and for this purpose baths will be found invaluable.

Amongst the mineral tonics and alteratives I may mention the biniodide of mercury. It is almost the only mercurial I habitually, so to speak, exhibit in the squamous class; but, as has been justly observed by M. Ricord, the hydrargyrum is so merged in the iodide that it scarce deserves the former appellation. The different preparations of arsenic must be resorted to in many instances. Biett has strongly advocated the use of the "arseniate of ammonia," others the arsenite of potash, liquor arsenicalis, &c. For my own part I think the "iodide of arsenic," in doses of one-sixteenth to one-tenth of a grain in pill, a peculiarly nice form for its administration—in short, I look upon the iodides to be the basis of treatment in this particular class of skin diseases. The preparations of larch are decidedly tonic and alterative, and are peculiarly suited for internal exhibition in chronic cases occurring in elderly or atonic constitutions; they may be applied locally in psoriasis either alone or combined with the dilute citrine or other special stimulant ointment; I think they deserve a very high place. Hygienic requirements are indispensable in the treatment of these diseases, not only as regards the omission of non-stimulating diet, but also as to perfect cleanliness and promotion of the functions of the skin. Hence, as I have already mentioned, the value of baths is self-evident, more particularly in pityriasis and in the syphilitic sub-species, where sea bathing or tepid sea bath will frequently be attended with the best effects.—*Dublin Hospital Gazette, Jan.* 16, 1860, *p.* 24.

92.—ON THE PREVENTION OF PITTING IN CONFLUENT SMALL-POX.

By William Stokes, M.D., Regius Professor of Physic in the University of Dublin.

[The various modes previously employed, with a view of preventing pitting in cases of small-pox, may be thus enumerated:]

1. The puncture of the pustules when matured.
2. The application of nitrate of silver.
3. The application of oil, or of the *linimentum calcis*.
4. The covering the face with a solution of gutta percha, with collodion, or with glycerine.

[The first of these modes is best adapted to a benign form of the disease, in which the pustules, though numerous, remain discrete for a longer period than in the severer cases. As to the second mode, Dr. Stokes has had no personal experience: it will be most suitable, like the last, to milder forms of the disease. The third form Dr. S. has tried, but does not consider either application of much value, though the linimentum calcis is preferable.]

During the past five years I have used gutta percha and collodion in a considerable number of cases. These, however, were not by any means examples of the worst form of the disease. In most of them the crust came off in large flakes or patches, composed obviously of the dried exudations and the covering material, and leaving the skin uninjured. To render this treatment effective, at least so far as the exclusion of the air is concerned, it is necessary to renew the application at intervals of from twelve to twenty-four hours; for the covering seems to be repeatedly broken up by the advance of the eruption and the swelling of the parts. Some patients are greatly distressed by the feeling of constriction caused by the coating of gutta percha or collodion, and in general the treatment in question appears unadvisable where there is much vascularity, heat, or swelling.

Looking at the frequency of pitting on the face, as compared with that of other parts of the surface, it is not easy to account for it, unless by referring to the fact that, while the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain in a dry and heated state—first, from the action of the external air, and next, from the increased vascular action. Hard and hot scabs are formed, and the ulcerative process makes it way downwards to a greater or less degree. Some have held that the liability of the face to markings was to be explained by anatomical considerations. However this may be, it will be found that in cases in which from an early period certain portions of the face have been kept protected from the action of the air, and in a permanently moist state, pitting does not occur. This may be seen in cases of sthenic confluent small-pox, where, with the view of preventing the adhesion of the eyelids, poultices have been used over the eyes. In such cases it will be often found that every part of the face is marked, except those over which the little poultices had extended.

The application of poultices over the face appears to me to be the surest mode of preventing the consequent disfigurement. We should commence their use at the earliest period, and continue it to an advanced stage of the affection. In most cases they may be applied even over the nose, so as to cover the nostrils; for these passages are generally so obstructed as to be for the time useless to the patient. If the nostrils can be kept pervious by injections, the poultices need not be applied over their orifices.

If the chances of marking are in proportion to the activity of the cutaneous irritation, we may hold that this method should fulfil three important indications of treatment—

1. The exclusion of air.
2. The moderation of the local irritation; and
3. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

The value of this treatment, however, will, I feel convinced, be best seen in the inflammatory or sthenic form of the disease. The best poultice will be that formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with the gutta-percha paper or oiled silk. I have never had occasion to repent the adoption of this practice.

[Dr. Stokes sums up his paper with the following conclusions:]

1. That the chances of marking are much greater in the sthenic or inflammatory, than in the asthenic or typhoid confluent small-pox.
2. That, considering the change in the character of disease, both essential and local, observed during late years, we may explain the greater frequency of marking in former times.
3. That in the typhoid forms of the disease the treatment of the

surface by an artificial covering, such as gutta percha, or by glycerine, will often prove satisfactory.

4. That in the more active or non-typhoid forms, the use of constant poulticing, and of every other method that will lessen local inflammation, seems to be the best mode of preventing the disfigurement of the face.—*Dub. Quarterly Journal, Feb.* 1860, *p.* 111.

93.—LUPUS OF SEVEN YEARS' STANDING EFFECTUALLY CURED.

(Case under the care of WEEDON COOKE, Esq., Surgeon to the Royal Free Hospital.)

[Applications of arsenic and chloride of zinc no doubt stay the diseased action in this intractable disease for a time; and cod-liver oil, as well as arsenic, administered internally, have some beneficial influence; but the disease must depend upon some constitutional cause, without a knowledge of which our treatment can be only in a great measure empirical. The present case occurred in a lad aged seventeen, an oyster-dredger, at Maldon, in Essex. He has suffered for seven years from lupoid ulcers of the face, nose, and lips.]

At the time of his admission, the whole face was covered with either the cicatrices of old ulcers, or ulcers encrusted and dipping into the muscles beneath the skin. The columna nasi was destroyed, and the disease was encroaching upon the alæ. The upper lip was thickened and ulcerated. The scaly ulceration extended upwards to the inner canthus of both eyes. Added to these miseries, he was extremely deaf, and altogether presented a most pitiable and unsightly appearance. Mr. Cooke ordered an ounce and a half of lemon juice to be taken three times a day, meat and porter, with green vegetables; and the following lotion and ointment: bichloride of mercury, eight grains; hydrochloric acid, sixteen minims; water, eight ounces: to be applied three times a day as a wash, afterwards covering the parts with zinc ointment.

He remained in the hospital under this treatment, gradually improving, until August 17th, when he was discharged, with all the ulcers entirely healed, the deafness diminished, and his general health re-established.

In May, 1857, he again came to town, and was re-admitted, the disease having returned on the cheeks and upper lip. The same treatment was adopted, and this time he was well again in a month.

In July, 1859, he again appeared amongst Mr. Cooke's out-patients, the upper lip and one cheek only being affected with the scaly ulcers. He was enabled to stay with a sister in town, and was, therefore, not admitted, but placed under the same treatment—namely lemon-juice and fresh green vegetables, bichloride of mercury lotion, and zinc ointment. The cure was very rapid, the ulcers having well cicatrized in less than three weeks. His hearing also at this time was very greatly improved, and his general appearance so altered that he was scarcely recognised as the same unhappy youth who first came under notice in 1856.

Mr. Cooke states that there was no sponginess of the gums, but that the strumous aspect of the lad, and his occupation as an oyster-dredger at the sea, led him to prescribe lemon-juice as an antiscorbutic remedy, and, as it turned out, with the happiest results. Still, the adjuvant effect of the local applications must be by no means slighted or overlooked in the treatment of similar cases. Perhaps, however, the leaning generally is to depend too much upon local, and especially caustic, applications.—*Lancet, Dec.* 31, 1859, *p.* 662.

94.—ON THE TREATMENT OF CORNS ON THE SOLE OF THE FOOT.

By Holmes Coote, Esq., F.R.C.S., Assistant-Surgeon to St. Bartholomew's, and to the Royal Orthopædic Hospitals.

I have lately had under my care some cases illustrating the nature of this painful affection and its treatment. It may be necessary to remark that corns are technically termed "clavi," from a fancied resemblance to the head of a nail; but most persons know that they consist of thickened epithelium or cuticle, and that a small bursa is sometimes found between them and the subjacent parts. They proceed entirely from undue pressure; hence they appear on whatever parts of the feet a pair of badly-fitting boots or shoes press unequally and unpleasantly. The remedy is simple, and consists in the person so affected wearing boots with a sole as wide as the sole of the foot, of ample length, square at the toes, and with the upper leather soft and moderately loose. But this advice is rarely followed, fashion exerting a more powerful influence than good sense.

There is, however, a form of corn which is found on the sole of the foot, the pain attending which is so great that patients are at times unable either to walk or stand. Mr. Erichsen notices it in his work on Surgery. "It is usually of small size and round in shape, the neighbouring cuticle being always greatly thickened and hardened. It is extremely sensitive to the touch, the patient shrinking when it is pressed upon, as if an exposed nerve had been injured. On slicing it down with a scalpel, it will be found to be composed of soft, tough, and white epidermis, arranged in tufts or small columns, in the centre of each of which a minute black dot is perceptible. Each tuft appears to be an elongated and thickened papilla, and the black speck is a small point of coagulated blood which has been effused into it. Around the depressions in which each of these corns is seated, the hardened cuticle forms a kind of wall."—p. 439.

I have known ulceration to occur in this morbid structure, when a deep and foul sore, excessively sensitive, is formed. It may be healed by rest, but it recurs when the patient resumes the usual habits.

Now the cause of these corns will generally be found to proceed from a tense condition of some of the important tendons, that most frequently affected being the tendo-Achillis. When it is so contracted, the foot cannot be raised beyond a right angle; and it follows that the weight of the body is unduly thrown on the fore part of the sole of the foot, where the corn speedily forms. The contraction of the tendon may be so slight as to need careful examination for its detection; but so long as it exists the cause of the corn remains, and it will be found that any other measure less than the division of the tendon will be only palliative. The subcutaneous division of the tendon, its elongation, and the restoration of the foot to its normal bearings must be conducted on the usual principles of orthopædic surgery. The practice has been adopted many years at the Orthopædic Hospital; but it is not so generally known as, in my opinion, is desirable.—*Lancet*, *Dec.* 10, 1859, *p.* 584.

95.—ON ARNICA AS AN APPLICATION TO BRUISES.

By Mitchell Henry, Esq., F.R.C.S., Surgeon to the Middlesex Hospital.

It is gratifying to know that in the common arnica plant, and in the tincture prepared from it, we have a remedy of very singular efficacy, applicable in all cases, from the most trifling to the most severe. I have employed it for several years past in a vast variety of cases, and can speak of it from experience as unapproached in power by any other remedy or combination of remedies whatever. Many persons suppose that we are indebted to the homœopaths for the introduction of arnica; but this is quite a mistake, although it has been more prominently brought into notice by their commendations. Arnica has long been in use as a popular remedy in Germany, as well as in this country. In Ireland, a poultice of the arnica leaves (the leopard's bane) is a favourite application to the "broken knees" of a horse, and is a far more rational one than the stimulating "oils" so commonly used in England. To bruises, to allay the smarting of wounds after operation, to fractures, dislocations, and all similar injuries, it may be applied with implicit confidence in its power, to the exclusion of fomentations, cooling lotions, and leeches. The only objection that can be made to it is that it sometimes irritates the skin; but this proceeds from its being employed in too great quantity and strength: or it is possible there may be a peculiar idiosyncrasy which predisposes the skin of some persons to become irritated by it. I can only say, however, that such cases must be rare indeed, for I have never met with one. I never employ it weaker than two drachms of the tincture to the pint of water, or stronger than two drachms to the ounce of water, and this only in rare cases, and for a short time, until pain has abated. The most useful strength is four or six drachms to the pint, and this may be kept as an hospital preparation, and used indiscriminately. In cases of very severe shock, as in heavy falls, a few doses of the tincture in quantities of five minims, administered internally at intervals of four or six hours, and carefully watched, for it is a depressant, will give great relief.

A few examples will serve to illustrate the foregoing remarks. And, first, one from my own personal experience. Three years ago the top of my finger was caught and violently jammed in shutting a carriage door. In the course of a few minutes the nail got black, and blood oozed from beneath its sides, whilst the pulp of the finger swelled up into a distinct tumour from the effusion of blood, and I need hardly say the pain was excessive. I went to the nearest chemist's, and got a lotion composed of two drachms of the tincture of arnica in one ounce of rose-water, and applied this assiduously as fast as the lint wrapped round the finger became dry, at the same time taking care to keep the hand well elevated. In the course of an hour the pain had very much abated, and I added some water to my lotion. Before going to bed at night all inconvenience had ceased, except when the part was touched; the blackness rapidly disappeared, and in a few days my finger was perfectly well, without the loss of the nail, and in striking contrast to what usually happens in such cases.

Many of you will remember a man, lately discharged from Clayton ward, who had fallen from a scaffolding twenty feet high, and had alighted upon the broad of his back. His sufferings were extreme; he passed blood in his urine, and subsequently for two days had retention. Nevertheless, the arnica lotion applied to his back, and a few doses of the tincture taken internally, relieved him in a marvellously short space of time, although every surgeon knows that there is not a more troublesome or lingering affection than this general shaking or bruising from a heavy fall.

Another patient, not yet discharged from the same ward, had an obscure injury to the shoulder-joint, simulating dislocation, for which attempts at reduction under chloroform had been made before I saw him. The injury was probably fracture of the glenoid cavity of the scapula, and was followed by ecchymosis extending half way down the back, and by very acute pain. Here the arnica was no less useful than in the former case, and the man has now almost recovered the use of his limb. To all our fractures you know it is an established remedy, and you saw me lately employ it as a lotion to relieve the pain which followed an operation for the extraction of cataract. Some of you will recollect that very formidable case of fistula in ano which lay in Bird ward, and which required so many operations for its cure. Nothing alleviated the severe pain attendant on it so much as arnica. I am informed by our house-surgeon, Mr. Cresswell, that he paints over any black eye that may be seen in the surgery with the pure tincture, which has the effect in the early stage, of preventing the ecchymosis almost entirely, and, in a later one, of rapidly getting rid of it. But I might multiply these cases indefinitely. I am bound to say, however, that I have not as yet found it to be so efficacious in relieving the pain of cancer as we might have hoped it would be, and this probably arises from the fact that its action seems to be confined principally to the cutaneous and immediately subjacent textures. And this leads me to speak of its *modus operandi.* We know very little indeed of the ultimate action of any medicine or external application, but the probability is that arnica produces anæsthesia of the cutaneous nerves, and exerts some influence on the ganglionic nerves, which surround the blood-vessels, and regulate their action; for it certainly arrests the formation of thrombus and ecchymosis. However, whether this is theoretically correct or not, the practical effect of arnica is incontestable.—*Lancet, Dec.* 10, 1859, *p.* 579.

96.—ON THE TREATMENT OF WOUNDS.

By Mitchell Henry, Esq., F.R.C.S., Surgeon to the Middlesex Hospital.

Strapping and adhesive materials, except sometimes as tending to support and consolidate the sound textures, should be avoided:—metallic sutures, either of well annealed iron wire or of silver wire, being substituted for them. Metallic sutures excite no irritation, and may be fearlessly employed in every situation and to all textures. Unlike sutures made of vegetable materials, the holes through which they pass undergo no suppuration or ulceration, and they may be left *in situ* for an indefinite period—for three weeks if it be desired; indeed, it is not an uncommon event for us to lose a wire suture altogether, the patient leaving the hospital with it still in his body. The arnica lotion should be applied over the wounded part, which, when practicable, should also be kept elevated high above the patient's body; and the relief that these two measures afford will surprise persons who are not familiar with them. The only exception to the propriety of elevating the limb is in cases in which there is much loss of temperature from defective circulation, or a fear of sloughing from the severity of the injury. To limbs in this condition, thick layers of cotton wool form the most advisable application; but, in my opinion, no worse practice can be adopted than that which is too common—namely, the placing of the injured part in a hot poultice that it may "recover itself." The results of careful adjustment, and of attempts to save textures and parts, almost against hope, will amply repay the surgeon, who, at the same time, places his reliance in opium and wine as constitutional treatment. I may here mention that when reaction and inflammatory fever set in, the tincture of aconite, when carefully watched, will exhibit the surprising power which it exerts over all forms of vascular excitement.

Suppurating Sores and Wounds.—Suppuration is a great evil, and may become a great peril to the patient. All applications that promote it are, therefore, to be avoided—such as hot fomentations of all kinds, poultices and ointments. Torn and disorganized parts will suppurate in spite of all that we can do. Our efforts must, then, be directed to rendering the process as little inconvenient as possible. The most scrupulous attention must be paid to supporting the suppurating textures by means of splints, strapping the sound parts of the limb, and applying graduated pressure, so as to limit the area of suppuration, and prevent the matter from burrowing. Next, the pus itself must be rendered as innocuous as possible by attention to cleanliness, and by disinfection when, as is most commonly the case, this also is required. Long ago impressed with the fact that erysipelas, diarrhœa, phlebitis, and low fever could often be traced to the foul condition of suppurating sores on the patient's own body, I have tried almost every disinfectant that has come before me, including of late the mixture of plaster-of-Paris and coal-tar, now so much eulogized in France. One objection to it, and to several others,—as, for instance, charcoal, one of the best of all,—is that they are unseemly, or smell disagreeably. No such objection, however, applies to a solution of Condy's fluid (the permanganate of potash), which, for two years past, I have largely and most beneficially employed in the case of all varieties of suppurating sores. A solution of from half a drachm to two or four drachms of the purple fluid, to the pint of water, will keep even the most offensive sore sweet and clean, and, as a general rule, produce no pain whatever. If it does, the solution must be made weaker, until the required strength is attained, for there is a great difference in respect to its tolerance amongst patients. Its benefits are, perhaps, more strikingly displayed in the case of burns than in any other form of suppurating sore. From the day that suppuration commences in the case of a burn or scald, under my care, it is dressed with lint saturated with a weak solution of this fluid, commencing with half a drachm to the pint of water, over which a layer of cotton wool is applied. I have no hesitation in saying that burns thus treated will get well better than under any other system that I am acquainted with. The smell, always one of the most distressing features in these cases, is very greatly lessened, if not altogether destroyed; and the patient, no longer tormented with a horrible odour constantly under his nose, regains his appetite for food, whilst he is spared the chances of constitutional affections from absorption of putrid matters.

Amidst the variety of cases thus treated which you have all seen, I need but single out, as an example, the case of Bridget C——, who for eight months lay in Bird ward. Hers was one of the worst cases of burn that I ever saw recover, extending, as it did, over nearly all the left side of the trunk and abdomen, by far the most perilous situations, as well as over the left upper and lower extremities. Those of you also who have watched, as I requested you would watch, that old woman from whom I recently removed a large polypus which had grown into the frontal sinus, and displaced the eye, will have observed how the serious typhoid condition into which she was thrown by the putrid discharge flowing back into the pharynx, was chiefly relieved by the assiduity with which the discharge was disinfected by injecting the two-drachm solution of Condy's fluid into the nares every four hours. So also you have often seen it employed as a wash for the mouth, as an application to destroy the fœtor of cancerous sores, and as an injection in uterine carcinoma. Chemical science teaches us that the permanganates are true disinfectants because they oxygenate, and do not act merely as deodorizers.—*Lancet, Dec.* 10, 1859, *p.* 580.

SYPHILITIC DISEASES.

97.—ON THE TREATMENT OF GONORRHŒA WITHOUT SPECIFICS.

By J. L. Milton, Esq., St. Martin's-le-Grand.

To bring into as narrow a focus as possible the arguments for and against all the systems of treatment as yet discussed, I shall take the liberty of reducing them to a few aphorisms; the pith of all wisdom so vaunted by Bacon. Dr. Johnson long ago told men that there was little original in the best book ever written, and I am sadly afraid mine is not one of the best. However, the collections of my experience are,

1. That the remedies yet enumerated, recommended, and dismissed to oblivion, often by their unnatural parents, though adequate to cure by far the greater number of cases, still leave many unrelieved.

2. That most of them are disagreeable, some dangerous, and some futile.

3. That there are no rules to guide us in distinguishing at the outset those cases which are, from those which are not, amenable to these remedies, the divisions given by Eisenmann Swedia, &c., having no bearing on this point.

4. That where so large a list of remedies is given, some attempt ought to be made to decide with accuracy in what cases each remedy should be tried, which as yet has not been done, so that every cure obtained is only an additional source of confusion.

5. That the reputation of injections has been injured by the want of any certain rules as to the relative value of the different substances employed, and the strength requisite in different cases, thus leading to the indiscriminate application of different substances of the same strength on the one hand, and on the other, to the equally indiscriminate application of injections of the same strength to cases not equally fitted to bear them.

6. That treatment has been made secondary to disputes about the

nature, sources, and history of this disease, and to speculations, for they deserve no better title, of the most transcendental kind, about the action of medicines; whereas the cure of disease ought to precede all other considerations; for however great may be the value of science, the welfare of man is a still greater object to every well-regulated mind.

I have spoken plainly on this topic. The trite generalities, the incessant repetitions of many writers are more likely to raise our admiration of their diplomacy, than to give us a clear view of their internal convictions, if they had any! They are worse than the vaculliations of a Greek chorus, where the temporary hero is in one scene a monster of stupid, ferocious cruelty, and in another a high-minded monarch of first-rate conservative principles, justly defending his throne and aristocracy; or where a slave is one moment a miserable martyr to a selfish oligarchy, and subsequently transmuted into a faithful servant, to be duly rewarded in good time with a wreath and a pension from the state.

Perhaps I am suffering from the malady which Livy tells us is chronic among authors, but such are really the only inferences I can draw.

Proposed Plan of Treatment.—After this rather uncomplimentary piece of preliminary discussion, the reader will naturally inquire whether I have anything better to offer in its stead. I reply that I must leave that point to his decision, and beg to submit for examination, first of all a plan of abortive treatment, and to demonstrate the results it seems to offer. To do this properly I must first take the liberty of dividing all cases of gonorrhœa into two classes, viz., those which admit, and those which do not admit, of such a plan.

Those, then, which seem most adapted for it are—

1. Cases where the patients present themselves before great pain and running have set it.

2. Patients who have had gonorrhœa previously, and in whom the present attack does not appear to be very severe.

3. Those cases where the patient is desirous of an immediate cure at any price, and who would rather go through anything for a day or two than have a long illness.

And before going to the details I must digress for a few minutes to combat an opinion which seems very prevalent, and which is that M. Ricord is constantly in the habit of using in abortive treatment of a similar kind, or, in other words, of preluding all measures with a strong injection of nitrate of silver. This may be an incorrect assumption, as I have no written authority for it, but I know I have repeatedly heard it stated, both in private and public, without contradiction. Now nothing could be wider of the mark. M. Ricord's abortive treatment, as laid down in his 'Traité Pratique,' consists of rest, low diet, thirty or forty leeches to the perinæum, followed by copaiba and *mild* injections of nitrate of silver, and he expressly confines his recommendation of a strong solution of this salt to those cases which begin "without pain, without any sign of inflammation."

Abortive Treatment of Gonorrhœa.—Before taking a single step, it is indispensably necessary to ascertain whether the patient can rest for the entire day after, and if not whether he is disposed to suffer considerable inconvenience. If he is unable or unwilling to do either, it is best at once to lay aside all thoughts of an abortive cure and refer the case to the second class.

But if this co-operation on his part can be obtained, the abortive treatment may at once be commenced. The patient should make water, and the surgeon then injects him with a solution of nitrate of silver containing five grains to an ounce of distilled water. The tube of the syringe ought to be at least an inch and a half long, so that the whole of the specific seat of the gonorrhœa is acted upon, and should consist of platinum or silver electro-gilded, as otherwise the salt soon corrodes the soldering and even the silver.

By limiting the strength of the solution to five grains, we avoid the severe pain which is caused by the strong solutions of this salt, and by retaining the injection in the urethra for several minutes, we can in almost every case attain equally as powerful an effect as by using a more concentrated solution for a shorter space of time. Should it fail to occasion either burning or smarting it may be withdrawn, and a solution of ten grains to an ounce employed instead.

The deep burning pain which now ensues is widely different from that produced by the salts of zinc, and is often accompanied by flushes of heat which fairly thrill through the frame. It is, however, generally soon relieved by bathing the penis with hot water, and a hot bath will for the most part effectually remove what the warm water has left undone.

The next step is to prescribe a dose of calomel, seidlitz powders, or citrate of magnesia, at least four grains, followed by purgative draughts of salts and senna every two hours until several loose stools are procured. The bowels should be completely scoured out, and no food allowed except a little warm tea or gruel, to assist the action of the medicines.

After every stool the patient should inject with a solution of sulphate of zinc from three to five grains in the ounce. The injection is to be kept in contact with the mucous membrane, till a slight sense of burning is induced, when it may at once be withdrawn. The penis should be bathed each time with water as hot as it can be borne, and the greater the heat the more complete the relief, not only to the pain produced by injecting, but also to the scalding, weight, &c.

The next day the discharge is generally thin and very small in quantity, the symptoms of inflammation have disappeared and the cure is mostly completed in a day or two by the use of the same means, the patient using the injection every time he makes water, and gradually raising the strength of it till it reaches ten grains to an ounce.

Mild aperients and low diet may also be continued. When this plan fails, the case may be referred to the second class, for I believe, that abortive treatment to succeed at all, must succeed at once.

Ordinary Treatment.—Every other case of gonorrhœa, every case in which the abortive treatment has failed, or in which it cannot be applied, and every case accompanied by excessive pain and irritability of the urethra, or of long standing, and attended by fixed pain on the under surface of this canal, might be placed in the second class. It is to these that I wish to apply a new treatment, substituting for all internal treatment certain salts of potass, with rhubarb, and injections so combined, graduated, and applied, as to act permanently but without much pain over the whole diseased surface.

Having convinced myself by experience of the inutility of the other preparations, the acetate was tried in the hope that the alkali in a diuretic form would act as well, without the disagreeable effects resulting from the administration of the liquor potassæ. I was for some time disappointed, but by combining it with spirit of nitre and rhubarb all the attendant disadvantages were overcome. The best proportions seem to be five drachms of acetate of potass with three drachms of spirit of nitre in a six ounce mixture; an ounce of which may be taken two or three times daily.

In more severe cases the chlorate of potass may be added, and as many disappointments and failures resulted at the outset, an available form of prescription may be of service. The best I have seen is formed by pouring five ounces of boiling distilled water upon two drachms of powdered chlorate of potass, and then adding three drachms of liquor potassæ, and three of the acetate.

When this solution is taken in doses of an ounce two or three times a day, an alteration in the discharge is soon noticed, indeed within forty-eight hours it is often materially diminished, becoming at the same time thinner, less coloured, and more mucous. This effect seems to be produced with equal rapidity in cases of long standing and recent ones, in women and in men, so that one might be tempted to look upon it as one of the true antiphlogistics in inflammations of this kind.—*Med. Circular, Oct.* 5, 1859, *p.* 165.

98.—*On the Treatment of Chordee.* By J. L. Milton, Esq., M.R.C.S.—After having tried almost every antispasmodic, including ether, chloroform, and sumbal, I can safely say that I have found nothing equal to camphor in the fluid form. In the solid state it does not act so rapidly; and, in fact, a remedy in a liquid form, as it must from its extremely fine state of division act more rapidly, is more suited for spasm. The spirit of camphor offers all the advantages sought for, and, given in drachm doses, is equally energetic and rapid in its action. I have also tried the essence of camphor, a patent medicine: it is a little more agreeable, and a great deal more expensive.

As in many other cases, the chain of morbid action should be broken or checked at once; and this is much more effectually done by giving two or three full doses, at short intervals without the least remission, than by small quantities, however long continued and regularly taken. The surgeon may, therefore, safely adopt the following plan:—

A teaspoonful is to be taken at night in water before going to bed; and every time the patient wakes with the chordee, let him at once rise and repeat the dose. In mild cases, one dose for a night or two is generally enough; and even in more severe cases the spasm is usually very much alleviated by the third or fourth night. So long as the gonorrhœa remains very bad, which ought never to be more than five or six nights, the patient may take a dose before going to bed. This remedy also answers extremely well in the bearing-down pains to which women are sometimes subject in gonorrhœa; but as these pains are generally worst in the daytime, the medicine may be given then; and here it is really a matter of convenience to use the essence of camphor, as it mixes well with any medicine they may happen to be taking.

In both cases, however, it must be given in full doses. A smaller quantity than a teaspoonful of either essence or spirit is of little service; and as this quantity is perfectly safe, it is best to insure success at once. In one or two instances it has produced some sickness, probably from something having been previously taken which had disordered the stomach. This, however, has not occurred more than twice or thrice, and the sickness was of very little moment. So that I only allude to the fact, lest any one might be discouraged by the appearance of this symptom from giving so valuable a remedy as the camphor really is.

The patient should be directed to keep the camphor in a tightly-corked bottle and in a cool place, and to have it at night by his bedside ready to take. It is best taken in plenty of cold water.—*Med. Circular, Feb.* 29, 1860, *p.* 132.

99.—ON SORES OF A NON-SPECIFIC CHARACTER.

By JOHN HARRISON, Esq., F.R.C.S., Albany Court Yard.

Of the sores which present themselves on the genital organs after promiscuous sexual intercourse, there are two principal kinds: the one distinguished as the true syphilitic or *indurated* chancre; the other, as the false, simple, or *soft* chancre. Both are contagious, but it is from the former only that secondary or constitutional symptoms arise, though it is the latter which is most liable to be attended by suppurating buboes.

True chancre is usually solitary, or, if there be more than one, they all appear at the same time. On the contrary, there are usually several simple chancres, but instead of occurring all at once, they may appear in succession. Simple chancres are much more frequently met with than true chancres—in the proportion, it has been reckoned, of about two to one. True chancre is not readily inoculable on the body of the patient himself, nor on the body of another person already affected with syphilis; simple chancre, on the other hand, is inoculable, not only on the body of the patient himself, but also on the body of any other individual.

Besides the sores just described, it is important to remember that ulcerations of the genitals are occasionally met with originating in simple causes, such as an herpetic eruption, or even a slight injury, abrasion, or excoriation. Such ulcerations, in consequence of a disordered state of the constitution, neglect of cleanliness, &c., may assume an unhealthy and obstinate character, and are liable to be mistaken by those not conversant with the subject for venereal sores.

An old patient called on me a few days ago to tell me that his wife was sinking from carcinoma uteri. This gentleman has experienced for years, at times, excoriations of the glans and irritable discharges from the urethra. He has, however, felt satisfied of the cause. On one occasion, an herpetic sore, from the irritation just mentioned, was a long time before it healed, and might have been looked upon with great suspicion. For the last two years, intercourse with his wife having been prohibited, he has been free from excoriations and urethral discharge.

The matter given out from simple ulcerations of the genitals, though in itself comparatively innocuous, may acquire an acrid and irritating quality, and thus become capable of giving rise to similar ulcerations in another person with whom the affected individual may have sexual intercourse. Nay, symptoms simulating the constitutional symptoms of syphilis, such as eruptions, sore-throat, &c., may, if I mistake not, supervene on such ulcers of the genitals. Whether these constitutional symptoms arise from absorption of the matter generated by the sores themselves is another question, just as the mode of origin of gonorrhœal rheumatism is a question.

The following is a case in which suspicious appearances presented themselves, though the cause was in reality of a very simple nature:—

A young lady having received an injury on the right labium, swelling and inflammation took place rapidly, and when I first saw her—some few days after the accident—a slough of considerable size was being thrown off from the inner surface of the labium. On separation, it left an ulcer with a sharp, well-defined edge, which proved troublesome, and took a long time to heal. At one period there was considerable thickening about it, so that I was almost inclined to look upon it with suspicion, and was nearly thrown off my guard by its appearance. The hymen was perfect, and there was that maidenly delicacy about the patient which was unmistakable. There was, in fact, nothing venereal about the sore. In such cases we must take every circumstance into account. I have frequently seen syphilitic sores in females of exactly similar appearance, and if I ever saw a chancre—so far as external or visible characters are concerned, it was this. What a grievous mistake might have been made!

Abscess in the loose cellular tissue of the labium, from slight mechanical injury, now and then takes place, which bursts on the inner surface, leaving a sinus that is occasionally a long time in filling up. In a case of this kind, where there is leucorrhœa, a mistake is liable to be made. I have seen condylomatous soft sores between the nates in females, in whom, from their rank in life, neglect or want of cleanliness was scarcely to be expected as a cause. In one instance, the condylomata were connected with an eruption which somewhat excited my suspicions. It is certainly open to doubt whether or not many such cases are really always so simple in their nature, and so free from syphilitic taint, as we might be led to suppose.

Sores occurring on the genital organs there is, then, reason to conclude are not always specific in character, though but too frequently considered as such. A distinction ought, therefore, to be drawn between simple sores and those which are truly syphilitic and liable to be followed by a train of after-symptoms, in consequence of the absorption from them of a specific poison into the system. It is most important, at the commencement of the treatment of sores on the genital organs, to determine, as far as possible, their character—whether venereal or non-venereal; and if venereal, whether syphilitic or non-syphilitic; or, in other words, whether the sore before us be one which requires for its cure merely simple, mild, unirritating local applications, with attention to the general state of the health, or whether it requires a specific plan of treatment. Those who have had much practical experience in this department of our profession must admit that this is a question which in many instances it is almost impossible to decide off-hand. From my own experience, I do not believe that we can, in all cases, discriminate at once and with entire certainty (whatever may have been written or said on the subject) those sores which will be followed by secondary symptoms, and those which will not, at whatever stage we may see them. Thus, it is frequently found that, in some states of the constitution, a trivial excoriation, having no venereal origin whatever—by this I mean, not derived from impure sexual intercourse,—if mismanaged, or irritated by the application of over-stimulating or unctuous substances, is liable to be altered in character by the unhealthy inflammation thereby excited, and made to assume a suspicious appearance; whereas a really syphilitic sore, scarcely attracting notice, being left to itself, often heals spontaneously, though it may be followed by secondary symptoms. It is therefore wrong to be too hasty in pronouncing sores syphilitic, and forthwith to administer mercury. In the worse alternative, nothing is lost by a little delay; whilst, if the originally simple non-venereal or non-syphilitic sores are treated as specific, by the uncalled-for administration of mercury, &c., much mischief may be done.

How frequently do we see a common graze or cut finger put on a bad aspect, with inflammatory induration surrounding it, and show little disposition to heal: the same thing often takes place with a simple abrasion or excoriation of the delicate investing membrane of the glans or inside of the prepuce. I am inclined to think that in many cases sufficient attention (particularly with reference to ulcers of the genital organs) is not given to the known indisposition, in certain states of the constitution, which a sore or ulcer shows in putting on the healing or reparative process, in whatever part of the body it may be situate. We ought not, therefore, to expect some ulcerations of the organs in question to differ from ulcerations elsewhere.

It is a fault too frequently committed to consider the generality of ulcerations of the genital organs we meet with in practice as venereal. We do not, I fear, take sufficiently into consideration the liability of these organs to the same mechanical and chemical injury as other parts. I have, over and over again, seen sores, which had been pronounced syphilitic, heal under the simplest possible treatment, no specific plan having been adopted; and I have had opportunities of watching the result,—no secondary symptoms have shown themselves in these cases. Had mercury been administered, and irritating applications been had recourse to, the result might probably have been different. The balance of the system would, at least, have been unnecessarily disturbed, and an undue susceptibility to morbific causes created. The simplest non-venereal sore may be surrounded by induration: were we, therefore, to give mercury for every sore with surrounding hardness we encounter, we should, indeed, frequently be prescribing the medicine unnecessarily. In strumous subjects, there is often induration around a sore. Such cases are but too commonly made worse by the injudicious administration of mercury.—*Lancet, Feb.* 4, 1860, *p.* 113.

100.—SUCCESSFUL TREATMENT OF GONORRHŒA AND GLEET WITHOUT COPAIBA.

By Weeden Cooke, Esq. (Being an abstract of a paper read at the Harveian Society.)

The author commences by stating it as the result of a very large experience, that copaiba in the treatment of gonorrhœa is not only unnecessary, but even, in a great many instances, injurious, and in all offensive to the last degree. As introductory to the subject of treatment, Mr. Cooke discussed the subject of the cause of chordee, the reason for the scalding of the urine, the distinction between true and spurious gonorrhœa, and the time when infection may or may not be apprehended. The conclusions he arrived at were—

1st. The chordee, in ninety-nine cases out of a hundred, was due to spasm, and not to effusion of lymph; and that cubebs, acting as an antispasmodic, formed the most efficacious remedy for this symptom.

2ndly. That the scalding was the result of the acid urine passing over the highly inflamed mucous surface of the urethra; and that this was to be remedied by the administration of the alkaline carbonates for the purpose of neutralizing the acidity of the urine, thus removing the principal cause of the continuance of the inflammation.

3rdly. That in all disputed cases, the true gonorrhœa may be known from the spurious by the presence of redness, heat, pain, and swelling, together with a purulent discharge, more or less green and offensive; whilst a discharge produced by connexion with a person who was leucorrhœa, or has recently been confined, will not be accompanied with these inflammatory signs and the discharge will be milky in consistence and colour, differing much from the thick purulent discharge of the veritable gonorrhœa.

4thly. The time when infection may or may not be apprehended was discussed in its incubative stage, and at its close. A case was given, showing that the disease may be caught from one person, and not communicated to another two nights after, because the purulent discharge had not commenced at the time of the second intercourse. Respecting infection at the close of the disease, the author had been enabled, from experience, to establish as a law for his own guidance, that gleet—i. e., a mucous discharge from the urethra, consequent on gonorrhœa—does not set up gonorrhœa in another person: but that, whilst any pus is to be found in the discharge, there is probability of infection.

Passing on to the subject of treatment, Mr. Cooke said that, upon inquiry at the London Custom House, he found that 118,396 pounds of copaiba were admitted into the Port of London only, during the first ten months of the year 1859. If this be administered at the rate of half a drachm three times a-day, and supposing each person takes it for three weeks, we have here copaiba enough to treat 473,584, or close upon half a million persons, and that in ten months only of the year. Considering how often it fails to cure the disease; how frequently it is rejected by, or at any rate, disorders the stomach; how tell-tale and disagreeable is its odour from the mouth and skin; how occasionally it produces a papular eruption all over the body; and that, in many instances, swelled testicle and stricture may be traced to its irritating influence, while gonorrhœa, rheumatism, and ophthalmia have been attributed to its administration; "considering all these objections," the author remarked, "is it not extraordinary that this disgusting medicine continues so long to hold its ground?" The abortive treatment by strong injections of nitrate of silver had proved a failure because in some instances inflammation of the bladder had resulted. The treatment by diluents was slow in its action, and not readily employed by persons engaged in active business. That by diuretics was scarcely more successful; whilst the administration of saline aperients was generally attended with an aggravation of the ardor urinæ as well as chordee. The treatment which had been most successful in the author's experience was the chemical treatment by the alkaline carbonates, given with a view of neutralizing the acid in the urine. Thus one great source of irritation was removed from the inflamed urethra, and the subsidence of the inflammation, which nature would effect, was allowed to take place. As auxiliaries, especially when there is œdema of the prepuce, lead lotions, and elevation of the penis against the abdomen, were commended. The inflammation having subsided and a muco-purulent discharge being left, the author had found, after giving trial to all the injections which have been at any time in vogue, that the chloride of zinc, introduced into this branch of practice by Mr. Lloyd, of St. Bartholomew's, was the most efficacious of any in curing the disease, and that with less discomfort and in a much shorter time than by any other means. Since employing this treatment he had had little, if any, orchitis amongst his patients. The strength of the injection he most commonly employed was two grains to the ounce, but in some instances one grain to the ounce is sufficient. Whilst advocating this treatment in persons of healthy constitution, it was necessary to completely change it in others. In the strumous, in the dyspeptic, in those of dissipated habits, and where the diseased person is an old offender, the alkaline carbonates are not called for, because either the urine is not acid or the inflammation does not run high. In such cases the tincture of iron, or sulphuric acid and bark, or gentian, or calumba, may be advantageously employed from the commencement; and the chloride-of-zinc injection in these cases is also of the utmost value in rapidly overcoming the disease.

Respecting diet, the author considered that after the subsidence of the inflammatory symptoms scarcely any restriction need be enforced, and that beer or wine in moderate quantites may be advantageously used by those who are accustomed to these beverages. He had found long-established cases of gleet yield readily to the chloride-of-zinc injection, accompanied with tonic treatment and generous living.—*Lancet, Jan.* 28, 1860, *p.* 92.

DISEASES OF THE EYE AND EAR.

101.—ON DIVISION OF THE CILIARY MUSCLE IN GLAUCOMA, &c.

By Henry Hancock, Esq., F.R.C.S., Senior Surgeon to the Royal Westminster Ophthalmic and Charing-Cross Hospitals, &c.

[Mr. Hancock regards glaucoma as depending upon an arthritic condition of the blood, and that the bloodvessels, sooner or later, become diseased and changed in structure, in the same way as the vessels and valves of the heart are affected in arthritic disease.]

According to Bowman, these vessels, in recent cases, excepting small aneurismal swellings of the retinal capillaries, seem to have a healthy appearance, and are free from atheromatous and fatty degeneration, but in the more advanced cases these changes are met with. It is not unreasonable, therefore, to infer that, though in the earlier stages there may not be any appreciable change of structure in the minute vessels of the retina and choroid, still they are in an unhealthy condition, predisposed to regeneration, and less able to resist pressure or to overcome any interruption to the circulation of their contents. Accordingly they form little capillary pouches in the retina, which frequently give way, the blood either spreading amongst the elementary structures of the retina, or, bursting through the hyaloïd membrane, forms clots in the vitreous humour, whilst the larger vessels of the choroid, becoming varicose, by their pressure outwards cause absorption of the sclerotica, and produce staphylomata of that coat of the eye. Consequent upon these changes in the bloodvessels, the humours of the eye are affected; the vitreous humour, containing blood-discs and fibrous shreds, assumes a yellowish tinge, whilst the lens has the same or a greenish tint. As the disease progresses, effusion takes place within the eyeball, rendering it exceedingly tense and hard by the resulting intra-ocular pressure, which, acting upon the ciliary nerves and retina, causes intense pain, and ultimately total blindness.

A difference of opinion appears to obtain as to the exact seat of this effusion. Mackenzie, for instance, (p. 896, 4th edit.) says: "The dissolved state of the vitreous humour, which my dissections of glaucomatous eyes lead me to consider as forming part of this disease, is always attended, at least in the middle stages of glaucoma, by an abnormal firmness of the eye to the touch, evidently arising from over-distension of the tunics." Baron Von Gräfe, according to Wolfe, ('The Lancet,' Dec. 10, 1859,) considers acute glaucoma to be a choroiditis, or an irido-choroiditis, with an infusion into the aqueous and vitreous humours, the augmentation of which causes intra-ocular pressure, thus producing excavation of the optic nerve and irido-plagia; whilst we read in Hulke's description of Bowman's dissections that, so far from the vitreous humour being dissolved or loaded with fluid, it is of remarkable consistence, and does not flow readily. When viewed by the ophthalmoscope, a glaucomatous eye usually presents a dilated state of the retinal veins, which are often tortuous and turgid with blood. Small ecchymoses are seen scattered over the surface of the retina, and occasionally small blood-clots in the vitreous humour, with pulsation of the arteria centralis retinæ, and excavation of the optic papilla.

In June, 1856, Baron Von Gräfe performed his celebrated operation of iridectomy for the first time. "The mode in which he performs it is by passing a Beer's cataract knife into the sclerotica, about a line's distance from the margin of the cornea on the outer side, and, having directed it forwards into the anterior chamber, cutting upwards so as to make an incision nearly half an inch in length. Through this the iris readily prolapses, or, if it do not, is readily drawn by forceps, and a portion of it, varying from a fifth to a third of its whole, is cut away. The remains of the prolapsed iris is left in the wound."

How the removal of one-fourth or one-fifth of the iris will cure a disease the pathological appearances of which I have just given you, has never been satisfactorily explained. It is said to be very successful, although its warmest advocates are by no means agreed as to the *modus operandi* of the operation. According to Hulke, "Von Gräfe conjectures that the excision of the portion of the iris removes pressure or tension by the diminution of the secretory surface of the iris by the relaxation of the tensor muscle of the choroid (ciliary muscle), and the influence exerted over the circulation of the choroid." According to Jordan, he maintains, "that the relation between the two chambers is altered, and that a removal of a portion of the iris tends to restore the communication desired;" whilst, according to Hildige, he asserts, "that by excising a portion of the iris, the secreting surface is diminished, and its secretion thereby reduced to a minimum."

Mr. Bowman supposes that the primary relief of the internal tension results from the escape of the aqueous humour at the time of operation, and its continuous trickling; that "as the wound becomes united, the gap in the iris allows the aqueous and vitreous humours to come together, of course with the intervention of the delicate hyaloid and suspensory ligament; whereas the iris was before an effectual barrier between them, as shown by the researches of Cramer, Donder, and others." He conceives the result of excision of the iris probably is, "the redundant fluid effused behind and mingled with the vitreous humour, causing it to compress the retina, is permitted to transude into the aqueous humour, and then to escape through the cornea (a road not previously open to it); or by being absorbed by the vessels distributed on the anterior surface of the iris. As this removal of effused fluid gradually takes place, opportunity is given for gradual restoration of the aqueous humour in its natural quantity, and for the return of the lens and iris to their proper positions, whilst the globe acquires more firmness without becoming tense."

Mr. Critchett, on the other hand, considers that "time is allowed for the adjustment of the normal tension, and a sort of safety-valve is left for a time, to prevent such equilibrium from being again disturbed."

You will observe from the preceding quotations that although Von Gräfe conjectures that the excision of a portion of the iris, besides other results, removes pressure or tension by the relaxation of the ciliary muscle and the influence exerted over the circulation of the choroid, the prevailing opinion is, that blindness in acute glaucoma mainly depends upon intra-ocular pressure from effusion, and that the operation of iridectomy, by evacuating the fluid, diminishing the secreting surface of the iris, and allowing the contact of the aqueous humour with the hyaloid membrane, effects a cure. If this opinion be correct—if the great object for attainment be simply the removal of intra-ocular pressure, it is extremely difficult to discover the superiority of iridectomy over "paracentesis oculi," as practised by Reverius in the year 1679, or in our own time by Middlemore and Desmarres; neither is it clear how, by the excision of one-fourth or one-fifth of the iris, the effusion can be reduced to a minimum, since three-fourths of the iris, with the whole of the choroid, remain. And the same doubt attends the third hypothesis, that relief is obtained by allowing the aqueous and vitreous humours to come together (the delicate hyaloid intervening); for if the assertion of Cramer, Donder, and others, that in the normal condition of parts the iris is an effectual barrier between these two humours, is correct, we can scarcely understand how the mutilation of so important a constituent of the eyeball as the iris, and the substitution of an unnatural for a natural relation of parts, can lead to the restoration and subsequent preservation of a normal and healthy performance of function. The following question may also fairly be asked: If the relief, as conjectured by Gräfe, results from the relaxation of the ciliary muscle, and the influence thereby exerted over the circulation of the choroid, why should we seek for this relief in a roundabout manner, by removing a large portion of the iris, and producing permanent disfigurement and the objectionable results attending this procedure? Why should we not at once attack the ciliary muscle itself?

I differ from those who regard acute glaucoma merely as a choroiditis, or an irido-choroiditis, with infusion into the vitreous and aqueous humours, as they seem to me to regard results as causes. I believe that glaucoma, whether acute or chronic, is essentially a disease of the blood and blood-vessels, and that the effusion or infusion, as may be described, is the result of this condition, which if not arrested, sooner or later, destroys sight. I do not, therefore, believe that any operation will of itself cure glaucoma, but that, by removing the impediment to the circulation through the blood-vessels of the choroid and retina, the disease may be arrested until, if not too far advanced, it may frequently be cured by the aid of constitutional remedies.

I have observed, what I have not found noticed by any previous writer, that in acute glaucoma the eyeball is constricted and marked by a circular depression at the point corresponding to the ciliary muscle, whilst the vessels around this part are gorged to a great degree. The eyeball is elongated in its antero-posterior diameter, and the cornea lessened in all its diameters, and rendered more conical than natural; whilst, when the patient turns his eyeball sideways, irregular bulging of the sclerotica (staphyloma) is exposed to view. In one or two cases, also, in which I performed iridectomy, the pupil was dilated to excess, and the iris so tense and rigid that it resembled a piece of cat-gut, and could with difficulty be drawn through the wound. It is not at all clear how regular, equal pressure from fluid within the eyeball can, *per se*, produce cupping of the optic papilla, pulsation of the retinal artery, aneurismal swellings of the retinal veins, a varicose condition of the choroid veins, &c.

Equable pressure from confined and compressed fluid exerted in all directions from within outwards, would, if uninfluenced by extraneous circumstances, tend rather to stretch the retina and choroid, and thus prevent the cupping of the optic papilla and bulging of the choroid. We can, however, readily understand that, the lateral expansion of the eyeball being, in a great degree, prevented by the constriction of the ciliary muscle, the force of the compressed fluid acts more powerfully in the antero-posterior direction; hence the puckering and cupping of the retina, the irregular bulging of the choroid, the alteration in the shape of the cornea, and the elongation of the eyeball.

Having carefully marked these several changes, and studied them in conjunction with the appearances observed by the ophthalmoscope in the interior of the eyeball, and with those seen on dissection, I compared them with the normal anatomy of the eye. I directed my attention to the connexion between the inner elastic layer of the cornea with the ciliary muscle, and considered how the vessels from the choroid pass through this muscle to reach the iris, the peculiar arrangement of the vessels of that latter organ, and of the choroid (especially of the choroidal veins) with regard to the ciliary muscle. I regarded also the relation between that muscle and the ora serrata of the retina, as well as the distribution of the retinal vessels close to their junction.

All these considerations led me to suspect that the ophthalmoscopic and pathological appearances of the blood vessels were greatly enhanced by, if not, in some instances, entirely due to, the obstruction of the circulation caused by the undue and excessive constriction exerted upon them by the spasmodic or extreme contraction of the ciliary muscle, analogous to the spasm so often observed in the muscular fibres of the urethra, as well as in the sphincter ani muscle in certain affections of those parts.

This supposition was strengthened by the character of pain so often described to me by patients as ushering in the attack of acute glaucoma: for instance, a lady, to whom I was called by Mr. Jackson, informed me that having been exposed to a very strong light at a party given by one of the foreign ambassadors, she felt, upon her return home, as though she had received a violent blow upon her eye, followed by excruciating spasmodic pain, which lasted for several hours

From these facts, I was led to hope that by cutting the muscle across, as we divide the sphincter ani under analogous circumstances, I should not only get rid of the effused fluid and relieve the constriction of the different parts connected with the ciliary muscle, but at the same time, by removing the impediment to the circulation of the blood, favour the return of the vessels to their normal condition, and so prevent a recurrence of the effusion into the eye; and I was the more inclined to make the trial inasmuch as, whilst failing, after the most careful study of what had been written of Gräfe's operation, for and against, to discover the principles which regulate its performance, I found that even where most successful, it causes certain results which it is most desirable should be obviated. For instance—

1. The disfigurement resulting from the removal of a portion of the iris, and the formation of a coloboma iridis.

2. The removal of one-fourth or one-fifth of the iris.

Whatever difference of opinion obtains with regards to other points connected with iridectomy, there does not appear to be any on this. All agree that the smaller the quantity of iris removed, the better. "By the excision of a portion of the iris, the edge of the lens, with its suspensory ligament passing in front of the vitreous humour to the ciliary process, is exposed to view; therefore, to remedy this inconvenience, Mr. Bowman makes an incision above, because he believes that the cover thus given by the upper lid to the margin of the lens, which has been exposed by the removal of the iris, contributes to the perfection of vision."

3. The loss of the power of adapting the eye to near objects, which it in some degree retains in chronic glaucoma.

"The exercise of this power depending upon the increased curvature of the lens in the pupillary area from the pressure of the iris on its margin,—in action which becomes almost, if not quite, impossible when part of the iris is excised."

By the operation which I am about to propose to you, these inconveniences are avoided. It is very simple, and may be performed easily and quickly.

I introduce a Beer's cataract knife at the outer and lower margin of the cornea where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided, whilst the accumulated fluid flows by the side of the knife. This procedure is rarely followed by bad symptoms. In one case there was inflammation, but it was reduced without difficulty. The operation appears to me to present the following advantages:—

1. It obviates the objections to iridectomy.

2. It relieves pain by the removal of the constriction of the eyeball, and the consequent pressure upon the nerves from the undue contraction of the ciliary muscle.

3. By it, the accumulated fluid is evacuated, and, the impediment to the circulation through the blood-vessels being got rid of, they are placed in a favourable condition to recover their normal state; and the probability of a recurrence of the effusion is greatly diminished.

4. By the situation and oblique direction of the incision, a free drainage of the fluid is provided for.

5. The iris is but slightly wounded, and the pupil is preserved of its original size and shape, and in its normal situation.

6. The danger of wounding the lens is avoided.—*Lancet, Feb.* 11, 1860, *p.* 133.

102.—*Laceration of the Upper Lid; Great Difficulty in procuring Union.*—Every now and then, after operations for hare-lip and other plastic procedures, on removal of the sutures, the parts again separate, to the great disappointment of both patient and surgeon. The following case may serve as a good model of the persevering treatment which ought to be pursued under such circumstances:—

A healthy lad, aged 15, had the upper eyelid of his left eye torn by catching it on a hook. The laceration involved the cartilage, and extended from the free border of the lid, in a direction upwards for at least half an inch, causing a gaping wound. He was taken to St. Thomas's Hospital shortly after the injury, and the torn parts were united by sutures. The accident occurred on Friday, December 23. The sutures tore out, and on the Monday following he was brought to the Ophthalmic Hospital with a large and most unsightly wound. The house-surgeon to the hospital lightly refreshed the edges, and put in wire sutures. These again tore out, and the wound gaped as before. Hare-lip pins were now used, but for a third time the hopes of union were disappointed. On January 9, more than two weeks after the injury, Mr. Dixon had the boy put under chloroform and pared the edges freely, and having done so passed a hare-lip pin a little above the free edge of the lid and at a considerable distance from the margin of the wound, so as to give it good hold. The upper part of the wound was brought together by two points of interrupted suture. From this last (the fourth) operation most perfect results were obtained. The parts united firmly, and a fortnight later, when the boy came under our observation, only a linear cicatrix was visible, and there was no irregularity in the contour of the lid.—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 165.

103.—ON LACHRYMAL OBSTRUCTIONS TREATED ON MR. BOWMAN'S PLAN.

By T. Pridgin Teale, Jun., Esq., M.A. Oxon., F.R.C.S.

[Mr. Pridgin Teale has now for more than two years frequently practised the mode of treatment recommended by Mr. Bowman for the relief of lachrymal obstructions. He first describes the mode in which he has usually performed the operation.]

Mode of Operating.—Critchett's grooved probe is introduced vertically into the lower punctum, and then passed in a horizontal direction along the canaliculus, and, if possible, into the lachrymal sac, so that its point may rest firmly against the nasal wall of the sac. Should the punctum be too small to admit the probe, it may be enlarged with the point of a cataract-knife, or an attempt may be made to pass the probe through the upper punctum and canal. The entrance of the probe into a distended sac may, in most cases, be easily known; since pressure of the probe outside the sac renders it more tense, whereas little or no increase of tension takes place on pressing the probe if it has fairly entered the sac. In cases in which the sac is not distended, the arrest of the probe in the canaliculus external to the sac may be inferred, if pressure with the probe causes a dimpling of the skin over the tendo occuli. For slitting up the canal a common Beer's knife answers well; the point of which may be carried along the groove in the probe until it is arrested by the termination of the groove; at this stage, in order to free the knife from the groove, the probe may be rotated on its axis, without changing the direction of the knife, which may be then thrust onwards until it is arrested by the nasal wall of the sac. A sufficient opening may thus be secured into the sac.

This mode of dividing the canaliculus differs somewhat from that of Mr. Bowman, in which the canal is not slit up beyond the caruncle. He is able thereby to introduce into the sac his largest probe (No. 6) through the unwounded part of the canal. Mr. Bowman's mode, which I have occasionally adopted, has the advantage over the other of inflicting a smaller surgical wound. My reasons for generally adopting the complete division of the canaliculus are,

1st. That it is more easy to introduce the probe into the sac, both at the time of the operation, and, subsequently, should the probing be performed at long intervals.

2nd. That the patient suffers less from the introduction of the larger probes into the sac.

3rd. That a probe two or three sizes larger than Mr. Bowman's No. 6 can be used without difficulty from the commencement.

4th. That I have not hitherto observed any evil resulting from it.

After slitting up the canaliculus I have seldom found any difficulty in keeping it open, as in most instances the lachrymal passages were in a state of chronic inflammation consequent upon obstruction of the nasal duct, a condition unfavourable to immediate union. In the only case in which I met with much difficulty the punctum was strictured, but there was no attendant irritation of the mucous lining of the canal. In such a case it might be well to excite a temporary irritation of the newly-cut edges by the application of sulphate of copper.

I have seen Mr. Bowman use a convenient substitute for the grooved probe and cataract knife, namely, a narrow-bladed knife with a blunt point, which he introduces at the punctum and carries along the lachrymal canal to the extent required.

Entrance into the sac having been secured, one of the larger probes is passed down to the strictured part. If the obstruction yields easily, the probe is pressed on till it reaches the floor of the nostril. If, however, it does not yield to moderate force, it is better to delay further efforts for a few days. This allows time for the parts to become less swollen and irritable from the relief afforded by the free division of the canaliculus; and, at a second attempt the probe may be pressed more boldly against the strictured part.

The probes which I have used are of two kinds: first, the series 1 to 6 described by Mr. Bowman, which are uniform in calibre and are curved in two planes to correspond with the nearly spiral shape of the nasal duct; secondly, bulbed probes, the size of the bulb in two of them being equivalent to Bowman's No. 3 and 6 respectively; and a third probe, having a bulb two sizes larger than No. 6, which I have called No. 8. The bulb of No. 8 is of the size of the ordinary wire guage No. 15.

These bulbed probes are curved in one plane only, the narrow part above the bulb allowing them to lie in, and in some degree to adapt themselves to, the irregular course of the duct. The bulbed probes I find, perhaps from habit, more easy of introduction than Bowman's, and I think they give less pain to the patient. I prefer Bowman's larger probe when any considerable pressure is required to overcome an obstruction, as the direction of the pressure can be made thereby more vertically, and more in the axis of the nasal duct, than by the bulbed probes which are curved only in one plane. After the probe has been once passed through the nasal duct, the treatment is usually very simple. The probe may be passed a second time, if necessary, at the end of a week, and may be repeated weekly, or at progressively increasing intervals, as long as the epiphora continues to relapse. On first adopting this treatment I used the probe every second or third day; but, finding the frequent repetition unnecessary, and perhaps injurious, I have been gradually led to allow longer intervals to elapse between each operation.

In my earlier cases I used the styles with the ends tapered, bent, and at a right angle, formerly suggested, but generally laid aside, by Mr. Bowman. These were introduced through the divided canaliculus into the sac and nasal duct, and allowed to remain for periods of four, eight, or twelve hours. They appeared, however, to retard progress by the irritation which they caused. Since the publication of Mr. Bowman's paper in November, 1857, in which he advised the "intermittent use of probes," I have never had recourse to these bent ones, though exceptional cases may occur in which they may prove of service.

When a patient has been free from epiphora for a month, he may be released from further treatment, unless there should be any relapse. —*Med. Times and Gazette, Jan.* 7, 1860, *p.* 9.

104.—ACUTE SCLEROTITIS AND IRITIS,

ILLUSTRATING THE ANTIPHLOGISTIC POWERS OF MORPHIA.

By J. Zachariah Laurence, Esq., F.R.C.S., M.B., Surgeon to the South London Ophthalmic Hospital.

[Perhaps the most practically important distinction in the ophthalmiæ is, whether the conjunctiva or the sclerotic is the part principally affected. In the latter, the pain is deeper seated, more paroxysmal, more agonizing. The treatment usually adopted is bleeding, blistering, and mercurialization. In the first of the following cases, the author at first administered the morphia, with the view of relieving the excessive pain, no further result being looked for. But he found that morphia really seemed to have powerful antiphlogistic powers, the decline of the disease soon following its free administration.]

Case. 1.—*Acute Sclerotitis—Morphia Treatment—Decline of the Disease in about Four-and-twenty Hours.*—S. S., a middle-aged woman was admitted to the South London Ophthalmic Hospital on Nov. 3, 1858. The sclerotic was intensely injected, the conjunctiva slightly; the "sclerotic zone" well-marked. She suffered such severe shooting pain in the eye-ball, eye-brow, and infra-orbital region, as to render her quite sleepless.

Nov. 3. ℞. Morph. hydrochlor. gr. $\frac{1}{4}$ every third hour. Warm water fomentations to the eye.

6th. Took the morphia regularly up to four p. m. yesterday, when she took the last powder. Towards the evening of the 4th the pain in the eye began to abate; now she feels but a slight aching in the eye, on exposure to light. The sclerotic vascularity has considerably diminished. She now recovered rapidly under the treatment of a slight conjunctivitis.

Case 2.—*Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than Twelve Hours.*—H. B., an elderly, but strong man, admitted to the St. Marylebone Dispensary, July 27, 1859. Sclerotitis of a week's duration characterized by intense vascularity of the sclerotic, and a "sharp, burning" pain in the eye-ball and forehead, with nocturnal exacerbation, rendering the patient sleepless. Suffering simultaneously from gout in the great toe. Has done nothing but foment the eye.

℞. Morph. hydrochlor. gr. $\frac{1}{2}$, 3tia. quaque horâ. Warm water fomentations to the eye.

July 29. Has taken the medicine as prescribed. Slept well, but not heavily, on the night of the 27th after midnight, when the hitherto severe pain in the eye left him. To-day, the vascularity of the tunics greatly diminished; the pain in the eye-ball, brow, and forehead gone, leaving but a trifling pain at the side of the nose. Bowels have not acted since the 27th. To leave off the morphia and take an ounce of castor-oil.

Aug. 1. Perfectly recovered.

Case 3.—Double Acute Iritis—Failure of Leeching and Mercurialization—Morphia Treatment—Decline of Disease within Four-and-twenty Hours.—E. P. was admitted to the South London Ophthalmic Hospital on Aug. 27, 1859, during my absence from town, and up to Sept. 10, when I first saw her, had been treated by leeching, blistering, mercurialization, and belladonna lotion for the previous three weeks.

Sept. 10. Iris discoloured; sclerotic deeply injected; pupils dilated (from the belladonna lotion); humours muddy; complaints of pain in the eye-balls and eye-brows, "like a rheumatic pain, of an overwhelming weight, of the light causing her great agony;" eye-sight very dim.

℞. Morph. hydrochlor. gr. ½, 4ta. quaque horâ. Warm water fomentations to the eyes.

14th. Took the first dose of medicine on the night of the 10th. The pain abated, and she expressly stated "very suddenly." She slept that night. On the following morning she could face the light much better. The medicine has made her feel very sick and drowsy. To-day, she complains only of a little "pricking and shooting pain." Her eyes are still dim and weak. but the sclerotic injection is nearly one.

28th. Since the last report has been taking the morphia in diminished doses, and subsequently a grain of quinine three times a-day. Her eyes are to all appearance perfectly sound; nothing remains of her disease but a slight haziness of vision.

Case 4.—Acute Sclerotitis—Morphia Treasment—Decline of the Disease in about Seven Hours.—B. L., aged 40, a working engineer, was admitted to the South London Ophthalmic Hospital on Sept. 24, 1859. Five or six years ago he was struck on the now inflamed eye by something from a forge-fire. He recovered from the accident in about a month. The eye has been inflamed, as it is now, for the last three weeks. It presents all the usual signs of acute sclerotitis; great sclerotic vascularity (the "sclerotic zone" well marked), excessive lacrymation, great pain (especially at night, rendering him sleepless) referred to the eye-ball, eye-brow, and temple, and compared by the patient to the sensation of "a weight hanging from his forehead, and pulling him down;" eye-sight "foggy;" over inner part of the cornea, a rust-coloured opaque speck, with a minute depression in its centre, evidenced the accident of five years back, but the most careful examination failed to detect any foreign body in the anterior chamber.

Sept. 24. ℞. Morph. hydrochlor., gr. ½, every third hour, watching its effects; warm water fomentations to the eye. Took the first dose about four p.m., felt sleepy about six p.m.; second dose about seven p.m.; the pain began then gradually to "die away;" the third dose about eleven p. m.—slept for three or four hours. The following day

(Sunday) at noon but trifling pain was felt, and he slept soundly that night.

28th. The case was reduced to one of slight conjunctivitis; all pain has left him; found his bowels confined from the medicine. To leave off the morphia, and take a purgative dose of calomel and colocynth, which completed the cure.

Case 5.—Traumatic Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than Four-and-twenty Hours.—C. H., aged 46, was on a Thursday evening engaged in Messrs. M.'s factory, pouring some molten iron into a sand mould, when a quantity of hot sand flew into his eye. He came to the South London Ophthalmic Hospital on Saturday, Oct. 1, 1859. With the exception of two minute particles of sand, which I removed with a spill of blotting-paper, all the sand had been removed by one of his fellow-workmen. I found him suffering from intense sclerotitis, marked by universal and high vascularity of the sclerotic and conjunctiva, great lacrymation and excessive pain in the eye-ball, compared by the patient to the "prodding of a knife," and rendering him quite sleepless.

Oct. 1. ℞. Morph. hydrochlor., gr. $\frac{1}{2}$, every third hour. Warm water fomentations.

Took the first dose of morphia about three p.m., and then regularly every three hours. It made him feel very drowsy, and that (Saturday) night he slept soundly. The violent pain was entirely gone on the following morning.

3rd. The case reduced to one of a simple conjunctivitis and treated by a purgative dose of calomel, which completed the cure.

Case 6.—Acute Sclerotitis—Failure of the Morphia Treatment—Recovery under Depletion and Mercurialisation.—E. S., aged 48, applied at the South London Ophthalmic Hospital on January 12, 1859. About twelve months before she lost the sight of the now-inflamed eye by a cork from a soda-water bottle. The consequent inflammation of the eye lasted for only a few days; but three or four months afterwards her eyesight began gradually to fade, and she can now only distinguish the outlines (but not the colours) of objects with the injured eye. About three weeks before applying to the Hospital she caught cold in the eye, which now presents the following signs:—Intense sclerotic and conjunctival vascularity ("sclerotic zone" well marked); pupil central of medium size, angular, destitute of contractility. Pain intense, referred to the right eye-ball and right side of the head, proceeding from the vertex downwards to the level of the ala nasi.

Jan. 12. ℞. Morph. hydrochl., gr. $\frac{1}{3}$, every fourth hour. Warm water fomentations to the eye.

15th. Pain and other symptoms unabated. She recovered slowly under leeching, blistering, and mercurialization.

Whether in this case the deeply diseased state of the eye, or the (too) small doses of the morphia, influenced the failure of the drug, must remain a matter of conjecture.

These cases I consider to establish an important practical fact, viz., that morphia is *per se* a powerful antiphlogistic, capable of curing these acute inflammations of the eye, in which up to the present time blood-letting, blistering, and mercurialization have been considered necessary. As regards loss of blood, all will be agreed on the propriety of dispensing with it, where it can be done so with safety. Again how constant an occurrence is it to see paroxysms of acute inflammations for a time apparently relieved by blood-letting, till the subsequent vascular reaction sets in, but to recur again and again, and require as many repetitions of this same objectionable remedy. I would further ask surgeons and physicians, What evidence have they that in the combination of mercury and opium given with a view of "putting the patient under the influence of mercury," as it is termed, is it not really the *opium* which does the good, and that the mercury and its action on the mouth may not be, to say the least, useless? * And I would finally ask the physicians of this country to test the powers of morphia in the treatment of the acute inflammations of the internal organs of the body.

If we seek for an explanation of the above very remarkable action of morphia in reducing abnormal fulness of the vessels of the sclerotic, we may find it in the relations of pain to vascular congestion. Pain has generally been regarded rather as the effect, than as the cause of the repletion of blood-vessels; but it is quite an open question, whether or not in certain classes of cases the order of things may not be inverted? Such may be the case in the inflammations of the sclerotic we have just been discussing. That, on the other hand, vascular congestion may react as a cause of pain, is not improbable. The theory I would submit is that the action of morphia in these cases depends on its known power of reducing nervous irritability, which may be viewed as the primary cause of the inflammation. In these deep-seated inflammations of the eye this view is very much borne out by the seat of the pain; this will be found to follow strictly the branches of the fifth nerve; indeed, the precision with which the patients themselves localise the pain is very remarkable, whilst we have further evidence of the nervous nature of these cases in the intense watering of the eye (dependent on irritation of the lachrymal branch of the fifth nerve). In this way I conceive the irritation is propagated to the vessels through the intervention of the connexions existing between the fifth and sympathetic nerves.—*Med. Times and Gazette, Dec.* 31, 1859, p. 651.

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* Again, mercury is presumed to have an "absorbing power" over plastic effusions, such as occur in acute iritis: here too it is a fair question whether the absorption of the inflammatory exudations is not rather a natural process, supervening on the cessation of the inflammation (such as we daily see in the absorption of divided cataracts after the operation by solution, as soon as the inflammatory consequences of the operation have passed off,) than any, if I may be allowed the expression, "mercurial" process?

105.—*Polypus of the Ear.—Treatment by Torsion.*—The patient was a woman, aged 30. She had had the disease six years. It had on one occasion been partly removed, but was now of large size and extended from the meatus. It was covered with cuticle and was quite firm. Mr. Bowman had punctured it, and evacuated some serum, and thus reduced its size; but it was soon again as large as ever. Mr Bowman said that it was impossible to state with certainty the point from which it grew, but it was probably the membrana tympani. Various methods of treating polypus of the ear were in use. Sof ones were removed in the same way as polypi of the nose, or their neck was ligatured by an ingenious instrument contrived for that purpose. He remarked that considering that the canal was narrow and curved, the latter methods were of doubtful value. It was necessary to use great caution in extraction; for if, as often happens, the polypus grows from the membrana tympani, this structure would be destroyed, the tympanic chain of bones might also be destroyed, and even death ensue by disease extending from the petrous portion of the temporal bone to the brain.

On Wednesday Mr. Bowman commenced the treatment of the case by slow torsion. He transfixed the extruded part by a needle, and then gave it a twist. He gave it another twist to-day. One effect on the tumour of this procedure was to render it livid and almost black, showing the immediate effect on the circulation of the mass. Mr. Bowman hoped that by this means he should cause ulceration of the neck of the tumour, and thus ensure its safe and complete removal.—*Med. Times and Gazette, Dec.* 3, 1859, *p.* 553.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

106.—THREE CASES OF INDUCTION OF PREMATURE LABOUR PERFORMED AFTER COHEN'S METHOD.

By E. Noeggerath, M.D., New York.

[The first patient was a German lady, of short stature, and with rhachitic lower extremities. She had been delivered in her first confinement by craniotomy.]

The pelvis was a model of rhachitic deformity. The promontory of the sacrum protruding forward and towards the left side of the pelvic cavity, diminished the antero-posterior diameter to $2\frac{1}{2}$—$2\frac{3}{4}$ inches, while the lateral diameter remained unchanged in extent; the outlet of the small pelvis was rather enlarged in consequence of the widely open pubic arch and the flattening of the sacral curvature. The whole basin presented but a very small degree of inclination. The general state of health of the patient was satisfactory. On Monday, 2nd June, about 11 o'clock in the morning, Dr. G. C. E. Weber and myself proceeded to perform the operation of inducing labour after the method of Schweighäuser, Cohen. The woman was placed upon her back with the nates projecting somewhat over the edge of the bed, and the feet supported by two chairs; an elastic catheter, of the ordinary size, was introduced into the mouth of the uterus, and pushed upwards, with the intention of bringing the instrument between the anterior wall of the uterus, and the fœtal membranes;—the point of it entered the womb to the extent of about four inches—then, with a syringe adjusted to it, we injected about seven ounces of water, heated to 90° or 100° Fahrenheit. As soon as the fluid touched the internal surface of the uterus, the woman complained of uneasy feeling in the abdomen, and we distinctly felt the uterus in a state of rigidity, which lasted for several minutes. After a time, the finger was removed from the external opening of the catheter, when a portion of the water was rejected through the instrument with considerable force. The withdrawal of the tube was followed by another escape of some water. During the following thirty minutes, the uterus was in an almost continual state of contraction with but very few and short ntermissions of flaccidity. Besides a slight degree of excitement and ittle headache, the woman's state of health, as well as her pulse, proved to be unchanged. Towards noon the pains grew stronger, but ess in frequency, with longer intervals.

At about seven o'clock in the night, the pain lessened in a degree that we thought it proper to make another injection. This was applied in the same way with the exception that we did not change the ordinary position of the patient in her bed, because the lips of the os uteri were already so much retracted by the previous pains, that the introduction of the catheter would meet with no difficulty at all. Whether the water was injected with a somewhat greater force than at the first time we cannot decide, but it all remained in the uterus, and the operation was followed by a sudden enlargement of the womb. Mrs. M. experienced a very distressing pain in her abdomen; much more so than she did at the former injections. It made such an impression upon her system that she fell into an almost unconscious state; the pulse sunk suddenly, so as to be scarcely perceptible; her face instantly became purple, and her breathing very much embarrassed. Half an hour later, when she recovered from these symptoms, she was seized with a violent chill, which lasted for nearly two hours. This was followed by a feverish condition, general heat, and a pulse of 130 in a minute. This alarming state gradually subsided, and a renewed succession of strong uterine contractions commenced. At seven o'clock a.m. of the following day, we were told that she endured almost incessant and very severe labour pains during the last night. At this time we found that the vaginal cervix had disappeared completely, the os uteri was opened to the size of a silver dollar, the well-filled bag protruded into the vagina with every recurring pain. Now we could ascertain, beyond question, a vertex presentation. At nine o'clock a.m. the os uteri dilated to its full extent, and the membranous cyst broke while it was protruded almost to the external orifice. At that time, the vertex was just engaged in the entrance of the pelvis. Passing over the very interesting peculiarities of this cranial parturition, it will be sufficient to say, that it required a full hour of time to bring the head down through the brim of the small pelvis, notwithstanding those tremendous pains, which are only witnessed with rhachitic females. But when the greatest circumference of the cranium had passed the upper part of the pelvis, then one of these violent pains was sufficient to drive the head through the whole cavity, and at once out of the labia externa up to the shoulders. The entire parturition, from the time of the first injection, was achieved in less than twenty-four hours.

The child, though born in a weak condition, was soon brought to the most satisfactory state of breathing and crying. After the placenta was removed by the ordinary manipulations, the uterus proved to be well contracted. The mother's condition was satisfactory, and has continued favourable.

[In the second case the pelvis was markedly rhachitic, and the antero-posterior diameter of the brim rather more than three inches.]

She was placed across the bed, the feet being supported by two chairs. By gently pushing the fundus uteri backwards, the os tincæ was brought more fully in a direction corresponding with the axis of the pelvis, and an elastic English catheter, with a metallic mandrin was introduced into the cavity of the womb, between its anterior wall and the membranes, as far as one and one-half inches, as it was impossible to push it any further without using considerable force. Through it about three ounces of warm water were injected and instantly expelled beneath the instrument. This was repeated with the same result. We now withdrew the catheter, and introduced it again in a somewhat different direction. By this manœuvre the catheter could be introduced considerably further upwards, and the full amount of the injected water was retained. Immediately after this, the uterus became hard and rigid, and the patient had to press downwards as if in labour pains, which lasted for about half a minute. The patient was now ordered to rise and walk about the room. No water was discharged. From half-past nine a.m., June 8th, when the first injection was made until half-past ten, a.m., she experienced four well-marked though feeble pains. From this time up to half-past nine, p.m., regular labour pains, increasing in strength and rapidity of succession were observed, and with almost every one of them a small quantity of water was discharged. Still their influence upon the os uteri was as yet very insignificant, being dilated to about the size of a two shilling piece. Towards midnight the pains grew very strong, and at about three, a.m., a large quantity of water was discharged with one forcing pain. Dr. Krackowizer saw the patient at four, a.m., and found the right scapula presenting (in front), head towards the left side; fœtal pulsations easily perceptible on the left side of the abdomen below the umbilicus. The patient was placed under chloroform, when the doctor turned the child by one foot and extracted it, except the head. The operation of turning was attended with some difficulties, and could not be performed as quickly as was desirable. The head itself proved too large to be extracted in the usual way, and consequently the forceps was applied, and thus the child was delivered. It proved to be still-born, and, although every effort was made to revive it, life could not be restored. The mother did perfectly well and was up after the ninth day.

[We omit the third case—and one previously published—as these suffice to explain the nature of the operative procedure.]

This limited number of observations is, of course, insufficient for a final discussion with regard to the value of the method, but added to the statistics already known, they will throw additional light upon the operation, and tend to determine its true position. The first man who conceived the idea of inducing premature labour by injection of water into the uterus, was Dr. Tac. Fried. Schweighäuser, of Strassburg. In his excellent work, 'Das Gebären nach der beobachteten Natur,' &c., Strassburg and Leipzig, 1825, he recommends to throw a quantity of warm water into the womb for that purpose. But, as he never seems to have practised it, we must attribute the whole merit to Dr. H. M. Cohen, of Hamburg, who first introduced this proceeding into practice. He called the attention of the profession to this method in a thesis written in the year 1846. Since that time we have through the different medical journals, accounts of upwards of sixty cases in which Dr. Cohen's directions were imitated, all of which are very favourable to the operation.

Let us now endeavour to compare these results with those of other methods. It would be a waste of time, to discuss anew the value of puncturing the membranes. What accoucheur would not prefer a method by which the membranes remain intact, thus avoiding all the trouble, and all the danger, connected with a dry labour? And as to ergot? I think no unprejudiced accoucheur will now resort to this remedy, with a view of inducing premature confinement, partly on account of the uncertainty of its operation (one failure in every fifth case, Krause), and partly on account of its generally admitted poisonous influence upon the fœtus. This remedy has had its day, and it ought now to be mentioned only from a historical point of view. The dilatation of the os uteri, by compressed sponge (Kluge's method) has met with invincible obstacles in many cases. In some instances, a sufficient dilatation of the os uteri was effected, but no pains followed; cases of this kind have been reported by Hobeau, Jæsche, Jacoby, and Barnes, and altogether about eighteen cases are reported where other means had to be employed, as the action of the sponge proved to be insufficient. Moreover, the application of compressed sponge is tedious both to the patient and the accoucheur. The same may be said of Busch's instrumental dilatation, and the method of Hamilton and of Riecke. The plugging of the vagina with scraped linen (Schöller), or with an animal bladder (Hüter), or with the colpeurynter (Braun) are, doubtless, more safe than the methods just mentioned, but altogether not free from inconveniences. The best of these contrivances is Dr. Braun's caoutchouc bladder-plug. Still, some cases are reported where it was unable to produce pains. Its chief drawback is the irritation of the vagina, and lower section of the uterus, in consequence of its application. Thus Professor Breit, of Tübingen, has published the case of a woman who died from inflammation of the internal genital organs effected by the colpeurynter. But for controlling hemorrhage, and promoting labour in cases of placenta prævia, the bladder-plug will always remain the remedy par excellence. Scanzoni's methods of inducing premature confinement by irritation of the nipples, or by irritation of the vagina and uterus, with carbonic acid, have met already with a number of failures counterbalancing entirely the amount of success obtained by them. The methods of Drs. Simpson, Merrem, Lehmann, Krause, which are intended to effect labour, by the introduction of a sound or a catheter with immediate removal, or with a view of leaving the instrument in the uterus, seem to be simple and effectual remedies for this purpose. But very few cases are reported in which they failed. Dr. Baun's latest proposition seems to be invented for the sake only of making a new invention He proposes to introduce a gut-string between the membranes and the inner surface of the uterus, the effect of which proceeding is certainly no other than that obtained by Krause's method. The use of galvanism (Radford, Simpson, Mikschik) is often very painful, not certain in its results, and tedious for the accoucheur, even should he happen to be in possession of an electro-galvanic apparatus.

Before entering upon a discussion of the position which ought to be assigned to the douche, we will briefly mention a case in which this remedy was used without making the least impression upon the pregnant uterus. The woman to whom we refer was received into the lying-in-hospital of Bonn, *enciente* with her first child, and at about seven months. She was of small rhachitic stature, and, although her spinal column was pretty straight, she measured not more than about four feet ten inches. Her pelvis was, therefore, not spacious and a pretty fair specimen of pelvis justo-minor, with an antero-posterior diameter of three and three-quarter inches. Under these circumstances, it was thought advisable not to let her go the full term, and the douche was selected for exciting labour pains in the thirty-sixth or thirty-seventh week of pregnancy. We had a large douche ascendante, which threw a powerful stream of water from a height of twelve feet. The basin on the top of it was filled with hot water (100° F.), and I directed the nozzle of the tube as near as possible towards the os tincæ. In this way the water was allowed to play against the lower segment of the womb twice a-day for fifteen minutes, and this application continued for a full month. This douche was applied at least fifty-six times, but in vain; not the slightest impression could be made upon the uterus, and not the least indication of uterine contraction could be obtained from beginning to end. She, therefore, was left alone till her full time, and as she had an exceedingly small child, and very strong pains, she was delivered even without the aid of the forceps.

But this is not the only instance of this kind. Dr. Krause, in his elaborate treatise on induction of premature labour, notices thirteen cases in which the douche was insufficient to effect labour (Scanzoni, three; Kowalsky, two; Michaelis, Grenser, Ziehl, Kilian, Ritgen, Goudoever, Dubois, Levy, each one), and not a few cases are mentioned where thirty to seventy applications were required to induce efficient pains (Diesterweg, Germann, Grenser, Arneth). We are in possession of accounts of about ninety-four cases in which the douche was used. Of this number, fourteen mothers sickened during the application of the douche—*i. e.*, one was taken with nausea, two with vomiting, three with hemorrhage, one with violent diarrhœa, two with vaginitis, two with metritis, three with fever; out of these ninety-three women, in whom the douche was applied, twelve died in childbed. This is a number unparalleled in the history of induction of premature labour. From eighty cases in which ergot was used, only three women died; from one hundred and thirty-five cases of induction of labour by tapping, eleven died; from ninety-six cases of intra-uterine injection, three died—viz., two from eclampsia, one from puerperal fever. Out of these eleven cases of death after application of the douche, six were owing to metritis. We are inclined to believe that some, if not all, of these metritides were caused by the douche itself. It is right to suppose that the congestion produced and constantly repeated by the act of throwing a full stream of warm water, as often as thirty or seventy times, in an interval of a few weeks, against the uterus, will at last become stationary, and pass through the different stages of an inflammatory process. It further appears from a perusal of the facts stated above, that the douche cannot be relied upon when applied for the purpose of inducing premature labour; it proved insufficient in about every sixth case, and had to be exchanged for another method. The uncertainty of action combined with the unfavourable results to the life of the mother are objections which cannot be denied. With regard to the fate of the children, it must be remarked that a considerable number of them were born in a cross presentation, altogether a larger per centage than with the other methods, a fact easily explained by the influence of a concussion (with an upward tendency) of the lower uterine segment and its contents. But as it is well known that almost all children who are artificially delivered before the end of the seventh month are still-born, it will be readily understood, that everything that has a tendency to produce malpositions does actually increase the per-centage of still-births. These considerations are modified when we examine the history of those cases, in which the stream of water, thrown from the douche, was directed so as to enter the os uteri. The manœuvre just mentioned has been recommended or executed by Kiwisch himself, by Arneth of Vienna, by Simon Thomas, by Trogher, by G. T. Elliot, A. K. Gardner, and many others. A glance at the results of the operation performed in this manner reveals a remarkable difference in the effects of the douche when applied in the usual manner. In most instances the result was striking, pains arising soon after the first application, while a few cases are recorded where death followed upon its administration. One instance of this kind is reported by Chiari, in which during the application of the douche, the patient was taken with convulsions, cyanosis dyspnœa, and died soon afterwards. A similar case is reported by Dr. Germann (see 'Monatschrift für Geburtsk.' xii., p. 193), who, after thirty-seven unsuccessful applications of the douche in the usual manner, introduced the mouthpiece of the chlysopompe into the os uteri one inch, and threw about one or one and a half ounces of cold water into the cavity of the womb. The patient perceived immediately afterwards a kind of tension and expansion of her abdomen. A few hours later, she had a most violent chill, and at once a series of the most forcible labour pains, of such a character that Dr. G. became alarmed for the patient's safety. The child was born, and twenty-four hours later the woman was a corpse.

It appears that the application of the douche, with the nozzle inserted into the mouth of the uterus, is a proceeding in many respects similar to that above described as Cohen's method, only less certain in its results, and, as it seems, more dangerous. We think that every reader of this article will conclude with us that the douche, used in the way first recommended by Kiwisch—*i. e.*, without introduction of the mouthpiece into the cavity of the neck, is a procedure, in the generality of cases, too slow, too uncertain, and, as it seems, too dangerous for both mother and child, to be relied upon, and it is now quite common to direct the stream of the douche into the uterus. But if labour is promptly induced in this way, this is mainly due to the water which entered by chance into the uterine cavity. It is really only by chance that the water passes between the uterine walls and the membranes, unless driven in by strong force; a proceeding too dangerous, as we have shown above, to be recommended. But if it is our intention to throw a certain quantity of water into the uterus, why not choose a method which is better adapted to the purpose, far more prompt in its effect, more safe to mother and child, easier for the operator, and less troublesome to the patient? We, therefore, propose to abandon the douche entirely, except in certain cases hereafter to be named, and substitute for it the injection of warm water into the cavity of the womb, by means of a catheter and a common syringe. I am sure that every one who has once tested Dr. Cohen's method, will be struck with the gentleness and promptness of its action, and the simplicity of its execution. In most instances, only one or two injections were required, and the average duration of labour from the time of the first injection was two days; not one instance is known of its failure, while the prompt recovery of the mothers in childbed, with the exception of those few cases where death resulted from eclampsia, gives us the best guarantee of the harmlessness of this procedure. Moreover, the apparatus required consists of such simple means, that every country-practitioner, residing in the smallest village, is in possession of them; they consist of an elastic catheter, a common enema-syringe, and a few ounces of warm water. The performance of this simple operation requires only a sufficient knowledge of the female sexual organs in the state of gestation, its execution is fully detailed in the history of the cases at the head of this article, and the only precaution to be taken is, to inject the water not with violence and force, but gently and slowly. But we meet, from time to time, with such a disposition of the internal sexual organs, that the introduction of a catheter is absolutely impossible, whether from a firm closure of the os, or from a location of the vaginal portion, so that it is out of our reach, in an upward or backward direction. Under such circumstances, we have to resort to a preparatory treatment in order to change the condition of the lower uterine segment, a treatment which in many cases may prove sufficient to induce efficient labour pains.

Of all means which may be chosen for this purpose, the douche is no doubt best adapted to our purpose. In acting principally upon the lower circumference of the womb, it is apt to soften the parts, to open somewhat the os, and to bring the vaginal portion more in the direction of the pelvic axis. We will further remark that Cohen's method ought not to be resorted to when induction of labour is required in case of uterine hemorrhage, from whatever cause it may arise. In such cases, nothing can surpass the caoutchouc bladder-plug (Braun's colpeurynter), which, introduced empty and filled with ice water, at once controls the bleeding by the double action of cold and pressure, and is almost sure to induce efficient labour-pains by its mere presence in the vagina.—*Contributions to Midwifery, p.* 9.

107.—TURNING IN CASES OF CONTRACTED PELVIC BRIM.

By Robert Barnes, M.D., F.R.C.S., Physician to the Royal Maternity Charity, &c.

[Dr. Barnes does not venture, so far as Dr. Tyler Smith, (who even now maintains the possibility of abolishing craniotomy in the case of living children,) though he believes there are resources in the obstetric art, safe to both mother and child, the relative merits of which, as compared with craniotomy, are as yet by no means sufficiently appreciated. He says:]

The question I propose to examine is this :—

Ought we to acquiesce in the dogma, that where the forceps fails, craniotomy is our only and last resource?

The affirmative answer involves two objections of serious moment. First, it condemns a multitude of children yet unborn to certain destruction. Secondly, it arrests, in *limine*, all improvement, all progress in the obstetric art, as bearing upon labour obstructed through contraction of the pelvis.

Humanity and science, then, conspire in urging us to hesitate long, and to examine patiently, before yielding our assent to a decision so revolting.

The case against turning and in favour of craniotomy in contracted pelvis may be stated briefly in this way. It is estimated that rather more than one-third of the children are lost under turning where the pelvis is of the ordinary size. It is urged that a living child may pass through a pelvis, the conjugate diameter of which is 3·25″, with or without the forceps. Now, it must be conceded that the main object of delivering by turning is to save the child's life. But in the case of a pelvis having a conjugate diameter of 3·25″, the operation is unnecessary. Its application must therefore be limited to cases in which the pelvis measures less than 3·25″. And, it is asked, what is the chance of the birth of a living child when the violence of compression is added to the ordinary risk of turning? Are we justified, for the sake of the very slender hope of saving the child under such circumstances, in subjecting the mother to the dangers of contusion, of laceration, of inflammation, which it is but natural to anticipate will follow upon dragging the child by sheer force through a narrow pelvis.

Such is the argument, endorsed by great names, that has to be encountered. It required no ordinary sagacity, and no common courage, to detect the fallacy of the argument, and to rebel against the authority that supported it. It required moral courage to dispute what seemed a well-settled rule of practice; it called for the courage of clinical enterprise to carry out a plan of treatment, the success of which was doubtful, the result of which might be disastrous. Under all this discouragement Professor Simpson revived the operation. If it be destined to resume a place in the obstetric art, this result will be mainly due to his sagacity and enterprise. But in according due praise to Dr. Simpson, the merit of the practitioners of former times must not be forgotten. The operation is not new, but revived. Dr. Simpson, presenting his papers on this subject in 1850 in a collected form, prefaced them with the following remark:—"Four additional sections or chapters were intended to be added—namely, one on the mechanism of the proposed mode of delivery as influencing the steps of the practice itself; a second, on the cases of obstructed labour chiefly adapted for delivery by turning; a third, on the best mode of conducting the operation in these special cases; and a fourth, or last, upon the history of the practice in ancient and in modern times."

I am not aware that Dr. Simpson has yet realized his intention of writing the last or historical section. But the deficiency has been ably supplied by Dr. Charles West and Dr. Ramsbotham. Since obstetric doctrines, like other things, sometimes revolve in cycles, it is interesting to examine the circumstances under which the operation we are now considering arose, and the phases it has undergone. For some time after the value of turning in order to deliver in cases of transverse presentation had been demonstrated by Ambroise Paré, the operation grew into favour, and was resorted to under a great variety of circumstances. It would appear to have been the general resource where artificial delivery was indicated on account of obstruction or urgent complications. The reason was simply that, the forceps being unknown, and the other instruments in use being so rude and inefficient, the hand was, and perhaps still is, really the obstetrician's best instrument. To it he therefore trusted. Many acquired great skill in turning. The operation was resorted to in cases of obstructed labour. Is was only after the introduction and vulgarization of the forceps that turning fell into disuse. And when the craniotomy instruments were perfected, for the purposes of lessening the head and extracting it by crotchet or forceps, craniotomy presented a solution of the difficulty so tempting by its facility that turning came to be nearly, if not altogether discarded. The practice, however, was partially maintained in this country by Dr. Denman; in France by Baudelocque and Lachapelle; in Germany by Stein and Osiander the elder. But although it was still distinctly advocated in 1833 by Osiander the younger, it may be said that the application of turning as a method of delivery in contracted pelvis is either emphatically condemned in the schools, or holds at best a very doubtful place. Now we are "trying back." And it is time. I cannot help declaring that craniotomy is carried to a frightful excess. As holding out a hope, at least, of lessening that excess, it has become a matter of urgent moment to direct our most anxious and candid attention to the applications of turning.—*Lancet, March* 24, 1860, *p.* 291.

108.—CASE ILLUSTRATING THE VALUE OF TURNING IN LABOUR OBSTRUCTED THROUGH COARCTATION OF THE PELVIC BRIM.

By Robert Barnes, M.D., F.R.C.P., Physician to the Royal Maternity Charity.

On the 20th of July, 1857, at eleven a.m., my assistance was requested by a surgeon in a case of convulsions. The patient was nineteen years of age, and in labour with her first child. She had been twenty-four hours in labour. Convulsions set in very strongly about two hours before my arrival. She had been bled to eight ounces. Her breathing was semi-stertorous; she was semi-comatose; the pupils contracted; face, body, and limbs enormously distended with fluid. She had passed about six ounces of urine, which "set" almost in a jelly on boiling, it was so loaded with albumen. The os uteri was of the size of a half-crown; head presenting; contraction fair. When I saw her again, at three p.m., there was more dilatation of the os; she had had one fit since. The os was rigid. Another fit now occurred. I ascertained that the antero-posterior diameter was considerably contracted; that the head was large, and firmly ossified; and that the long forceps could not be applied. In addition to the bony contraction, there was the further diminution of capacity from infiltration of the cellular tissue with serum. It was urgent to deliver. I did not think the case favourable for turning, and therefore lessened the head. Meconium had passed; and, although the condition of the patient did not admit of uterine auscultation, I thought it probable that the child was dead from the effects of blood-poisoning. The extraction of head by crotchet was tedious; the bones had to be much broken up. I am now disposed to think that the delivery might have been accelerated by turning after lessening the head. There was a fit almost immediately after delivery. The uterus contracted well; there was very little hemorrhage. I learned that the patient recovered well.

Second labour.—I had urged that, in the event of a second pregnancy, labour should be induced at seven or eight months. I heard, however, nothing more of the patient until the 31st of July in the following year, when she was again in labour at full time. I was then sent for on account of protracted labour. The head, it was represented, had been partially in the brim for seven hours without progress. The forehead was turned to pubes. I perforated at once, and delivered by crotchet. The woman recovered well.

Third labour.—Early last year, my renewed recommendation to have labour brought on prematurely having made an impression, Mrs. H—— came to me when pregnant. On the 1st of June, when it was calculated that she was quite seven months gone, I began the use of means for inducing labour. The os being closed, I inserted a sponge plug into the cervix, and ordered the uterine douche three times a day. On the 6th I removed the sponge, which had slipped out of the cervix. The os was now dilated to the size of a shilling, admitting of the head being felt. I now gave several doses of ergot at intervals until the 8th. The patient declared that the waters escaped freely towards evening; but at eleven p.m. I ruptured the membranes. The enlargement of the uterus was excessive, from the quantity of liquor amnii. This impeded free contraction. I applied a tight binder to support the uterus and promote the discharge of liquor amnii, which still impeded contraction. The head lay transversely, occiput to right ilium. Labour lingered until four a.m. of the 9th, when the os being sufficiently dilated to permit trial of forceps, I made the attempt, hoping that the smallness of the head might leave room for a pair with narrow blades; but the protrusion of the promontory frustrated this attempt. I then gave chloroform, but not to degree of surgical anæsthesia, and introduced my left hand to turn. I felt the cord pulsating. The right knee was seized, and turning effected by help of external pressure. The half-breech and shoulders passed with some difficulty; but to bring the head through the brim I had to use considerable force, so much that I hardly expected the child would be born alive. The head slipped through the narrow brim into the cavity of the pelvis with a sudden jerk. There was no difficulty at the outlet. The child, a male, was born asphyxiated; the heart was beating feebly; it made one or two inspiratory efforts. After five minutes' resort to the rotation-method, breathing was gradually established. Cold aspersions on the distribution of the respiratory nerve, and momentary dipping in warm water, completed recovery. The child did well. The placenta was extracted with some difficulty, as is not seldom the case in contracted pelvis. The mother recovered without a bad symptom. At the date of publication the child is thriving.

Commentary.—The preceding history needs but few remarks. The urgency of the mother's state in the two labours at term left no choice but to deliver by lessening the head. The narrowing of the pelvic brim precluded the use of the forceps. In both these labours the children were large, and the heads firmly ossified. An attempt to deliver by turning, instead of perforation, would, in my opinion, not have succeeded. The disproportion between head and pelvis was too great to hold out a reasonable hope of bringing into the world living children, and the state of the mother imperatively indicated resort to the method that was quickest and least severe. The question whether some little time might not have been saved by turning after lessening the head, may fairly be entertained. The result of the third labour is eminently satisfactory. But for the application of turning in conjunction with the induction of premature labour, this patient was doomed to conceive only to see her fruit perish in the act of birth. To have anticipated the period selected for inducing labour, so far as to secure a fœtus capable of passing through the narrow pelvis by the natural efforts, would have been to ascend beyond the probable limits of viability.

Here, then, is a case lying so clearly between the boundaries of the necessity for perforation or of the applicability of the forceps, on the one hand, and the induction of labour at a stage when the birth of a viable child is improbable, on the other hand, as to place in a striking light the value of the operation of turning. Turning accomplished what no other proceeding could accomplish. It saved the life of the child, whilst it added nothing to the risk of the mother. The case is a complete illustration of Conservative Midwifery.—*Lancet, April* 7, 1860, *p.* 341.

109.—*Turning as a Substitute for the Forceps in cases of Narrow Pelvis.* By Robert Jones, Esq., Strefford.—In a series of papers published some years ago, Dr. Simpson advocated the operation of turning in labours connected with narrow pelves, in preference to the use of long forceps or craniotomy, provided it be had recourse to early, as being attended with less danger to the mother, and as affording a greater chance of saving the life of the child. He states that morbid contraction of the brim is, whatever mode of delivery be adopted, liable to cause rupture of the uterus, especially when the labour has been allowed to be protracted, and the compressed tissues of the cervix have consequently been rendered friable; and that, owing to the elasticity of the cranial bones, great force might be used in extracting the head, without endangering the child's life, &c.

The following case is related as corroborative of these views. A few years ago, early in May, Mrs. Cook requested me to attend her in her forthcoming confinement. She is a little spare Irishwoman, aged 38, the wife of a coachman. She had then nearly completed the eighth month of her eighth pregnancy. She informed me that her first four labours were tedious and painful; the children were small, but born alive, and were still living; that, in her next two confinements, she was attended by two different medical men, both living at

Leintwardine; that in each the labour was painful and very protracted, and craniotomy was had recourse to in each. In her seventh confinement, which happened eighteen months ago, she was attended by a gentleman practising in Ludlow; this time the labour lasted thirty hours; it was left to nature, and the child was born dead.

I took this history as sufficient evidence of the existence of a morbidly contracted pelvis, and at once suggested the induction of premature labour. To this my patient stoutly objected. There was, therefore, nothing for it but to wait the natural time. Accordingly, early in the morning of the following 21st of June, I was sent for. I found she had been in labour all night, and for the four last hours the pains had been very powerful. The vagina was well lubricated, the sacrum very hollow, and the promontory greatly projecting forwards. The os uteri was well dilated, the membranes entire, and the child's head just entering the brim. With a trifling degree of force, I pushed back the head, carried my hand into the uterus, and brought down the feet without the least difficulty. Not so, however, with the head; for this required all or nearly all the strength I could use and keep up for some minutes. The head at length passed. The child was apparently dead; but the funis beat feebly, and at long intervals. Cold water being gently dashed on the face repeatedly, the child gasped, cried, and ultimately breathed well, and, I believe, is still living. The mother made a good recovery.—*British Med. Journal, Jan.* 14, 1860, *p.* 30.

110.—*Practical Remarks on Fœtal Auscultation.* By R. DRUITT, Esq., Licentiate of the Royal College of Physicians, London.—Of all the signs which distinguish the enlarged uterus from other tumours (though authors do not notice it), none is more valuable than the following: —The uterus, like other hollow viscera, has a regular peristaltic motion, continuous throughout pregnancy (and after delivery), and consisting in periodic contractions, which cause a moderate, but decided tension of the organ, and are followed by flaccidity and repose. No other tumour, not tympanitic, can do this; and during the fits of contraction the shape, dimensions, and outline, of the organ are unmistakeable. When about to auscultate, gently shampoo or roll the abdominal parietes over the womb, till you find it hard and resisting. That is the moment for auscultation. Be careful to put the stethoscope on the womb itself, and perpendicular to its surface thus isolated and defined; not merely in a vague way on the abdomen. Search carefully on the horizontal line on a level with the anterior superior spine of the ilium; beginning on the left side, then a little above and below; if unsuccessful go to the right side. Take care that your attitude is easy and produces no rushing noise in your ears; and if the woman be pregnant of a live child you will hear within a limited space, the heart-beat as distinctly before birth, as you can after birth, and just like it.—*Med. Times and Gazette, Jan.* 21, 1860, *p.* 58.

111.—REMARKS ON THE EMPLOYMENT OF UTERINE PESSARIES, WITH THE DESCRIPTION OF A NEW INSTRUMENT.

By E. Noeggerath, M.D., New York.

In recommending the use of pessaries in the treatment of prolapsus uteri, I am far from resorting to it in every-day practice, viz., that of diagnosticating prolapsus uteri, and prescribing a pessary at once. Nay, there are cases which do not justify instrumental treatment at all, while almost every single case demands a preparatory treatment before a pessary can be applied. The necessity of a careful examination and a full consideration of the complication present cannot be urged too strongly. The neglect of this principle is the common source of failure in the treatment of prolapsus. For the same reason, no physician should prescribe a pessary on the sole assertion of the patient herself, that she suffers from falling of the womb. I have frequently met with patients, who believed themselves to be subject to this complaint, who, upon examination, were found to have metritis or malpositions and flexions of the womb. It is obvious, that a pessary in this class of cases, would be injurious instead of beneficial.

The patient must be examined as well in an erect as in a horizontal position, as it often happens that a prolapsus disappears when the patient is lying on her back. After the presence of prolapsus has been ascertained in this way, the patient must be subjected to a thorough examination, while in a horizontal position. It is best to begin with the palpation of the abdomen, in order to get a knowledge of abnormities in the supra-pelvic and pelvic cavities. Hereafter the prolapsed portions themselves must be inspected, and the state of the anterior and posterior wall, and that of the womb itself, have to be taken into consideration.

Moreover, the color and condition of the respective mucous membranes have to be taken into consideration, as well as the presence of ulcerations, their different character, their seat in the cervical canal, near the orifice, or on the walls of the vagina. Hereafter the prolapsed portions have to be touched all around with the fingers. in order to ascertain their condition, and the possibility of full or partial reduction. In order to get a full view of the position of the uterus, it is well to introduce one or two fingers into that portion of the vagina which is inside of the pelvis. By examining through the rectum, we may ascertain how far it is involved in the prolapsus. Hereafter the situation and size of the womb has to be ascertained with the probe, and that of the bladder with the catheter. After this the parts must be pushed upwards, in order to examine the sexual organs inside of the pelvis and the pelvis itself. In those cases, where the neck of the uterus is not in sight, it has to be explored with the speculum.

The different forms which a prolapsus may represent are as follows:

1. One of the walls of the vagina may prolapse, without participation of the womb, viz.:

Prolapsus of the anterior wall of the vagina.

Prolapsus of the posterior wall. These cases are generally rded under the name of cystocele and rectocele vaginalis.

. Prolapsus of one or both vaginal walls, with partial prolapsus of the womb.

(*a*) Prolapsus of the anter-wall of the vagina and partial prolapsus of the womb.

(*b*) Prolapsus of the posterior wall of the vagina and partial prolapsus of the womb.

(*c*) Prolapsus of both walls of the vagina and partial prolapsus of the womb.

The cases of prolapsus of the anterior wall and the uterus are very often connected with the retroversion and flexions of the womb. The body of the womb is generally turned somewhat backwards, pressing upon the os sacrum and rectum. These cases, therefore, are very often complicated with very troublesome constipations of the bowels.

3. Prolapsus of both vaginal walls and complete prolapsus of the womb. This variety is the most commonly met with, because women affected with the disease very often do not apply for medical advice until twenty or even forty years have passed since its first start.

4. Prolapsus of the uterus. This is of very rare occurrence. The inferior portion of the womb, generally hypertrophied in a great measure, protrudes between the labia majora as a thin cone, which sometimes attains the length of three or four inches. As its lower end is rounded off, and perforated by the orifice, it resembles the penis of the male.

In most cases of prolapsus the lining membrane is the seat of superficial or deeper ulceration. The ulcerations coincident with prolapsus must be divided into two different classes, viz., those which are the consequences of idiopathic uterine disease, and those which are the result of mechanical irritations. This distinction is important with regard to treatment. The ulcerations from a mechanical cause are limited by irregular, sharp, callous edges, and their base is discolored with a brownish hue, yielding a dirty, thin, often very offensive secretion. The ulcerations from chronic metritis are of a more inflammatory character, inclined to bleeding, spreading rapidly on the slightest occasion, and very obstinate to treatment, unless the metritis has been subdued beforehand.

Other complications very often connected with prolapsus are *retroflexio*, *retroversio*, and *anteflexio*. Every complete prolapsus uteri is followed by *hypertrophy* of the organ, which attains in most cases the longitudinal axis, while at times the womb is considerably increased in thickness. In the first instances, the probe may be advanced into its cavity as far as five or seven inches. In other cases the cervical portion alone or one of the lips only are hypertrophied.

In consequence of the *displacement of the bladder*, always present in cases of prolapsus of the anterior wall of the vagina, the urethra is often covered with fungous vegetations, which at times attain the len half an inch in diameter. *Hernia recti* and *prolapsus ani* are of paratively rare occurrence, while *rupture* of the *perineum* is not dom. These and other complications have to be removed, as far possible, before the application of a pessary can be thought of. T treatment of some is very tedious, and demands a good deal of patienc from the attending physician and the woman herself.

Chronic metritis, hyperæmia and painfulness of the prolapsed parts, must be treated with leeches, sacrifications, anodynes, resorbents, &c. The ulcerations have to be cured thoroughly before a permanent retention of the womb can be thought of. It is perfectly contradictory to experience, that the reposition of the parts into the vagina is sufficient for the cure of these ulcerations, an opinion cherished by some of our very first obstetric physicians. The only complication which requires no treatment before the application of a pessary is simple hypertrophy of the womb.

The most efficient remedies for treating these ulcerations are nitrate of silver, acideum pyrolignosum, scarifications, removal with the knife of the callous edges, fomentations with lead-water, slight cathartics.

The *ulcerations* of the *vaginal walls* are of a very intractable nature; they are never benefited by the application of caustics, such as nitrate of silver; scarifications repeated every third or fourth day, and the applications of acid—pyrolignosum, answer much better. They often require twelve or eighteen months' treatment before a sufficiently firm scar has been attained. The ulcerations seated in or near the cervical canal must be healed up (at least as far as they spread over the lips) before a pessary can be introduced, while the treatment of the intra-cervical ulcerations may be continued afterwards with the speculum. It must never be forgotten, that all ulcerations which are touched by the pessary will increase and make the use of an instrument impossible. Only in those exceptional cases, where the ulcerations resist the most rational and persevering treatment, they may be covered with a piece of soft and dry lint, and a pessary introduced afterwards, and treatment continued intra-vaginam. In those cases where bodily rest can be resorted to, it is of great value for the cure of ulcerations; at any rate in treating these affections, the greatest cleanliness must be observed, the parts must be thoroughly sponged after going to stool, and they must be covered always with a clean piece of dry linen.

The use of a pessary seems to be connected with the greatest difficulty in those patients where prolapsus is complicated with both hypertrophy and flexion of the womb.

In the very first days of its application violent back-ache, a sensation of bearing-down and prolapsus of one of the vaginal walls, make their appearance. When examined, the body of the retroflected uterus is found very painful, and ulcerations appear on different places.

In these cases it is a good plan to elevate the retroflected womb by the uterine sound, thus fixing it towards the promontory. Then a pessary may be introduced and absolute rest recommended for some time. If this is not sufficient, the only means left, is to introduce a soft sponge behind the cervical neck, which, in many cases, does retain the prolapsed womb in its position. The sponge has to be removed, cleansed and reintroduced daily for some weeks before another application of a pessary may be tried, which at first must be applied in connection with the sponge. By a strict and indefatigable adherence to these rules, a pessary is finally endured without any inconvenience.

After a full consideration and treatment of the different complications, it is of the greatest importance to choose the right kind of instrument.

The requisites of a good instrument are as follows: 1. It must retain the womb in or near its natural position. 2. It must neither irritate the womb nor the vagina. 3. It must not interfere with the patient's moving round, sitting, or excretion of urine and fæces. 4. It must be composed of a substance, which resists the corrosive influence of the secretions from the genitals. 5. It be must constructed so as to be easily introduced, removed and cleaned by the patient herself. 6. It must be as cheap as possible.

The different pessaries may be divided into two sections, viz., those which support the womb directly, and those which support it indirectly, by elevating the vagina. Until late years, only the former class was exclusively applied, as this idea most naturally suggested itself at first sight. They are divided again into *stalked* and *unstalked*. Both are intended to give a direct support to the fallen uterus. Later researches seem to show that the chief and most natural support of the uterus was presented by the vagina, and in this view surgical operations as well as instruments were invented, and, as it seems, successfully applied for the cure of prolapsus.

The first man who clearly followed this indication in constructing his pessary, was Prof. Kilian, in 1846, and he called it elytromochlion—*i. e.*, vaginal supporter.

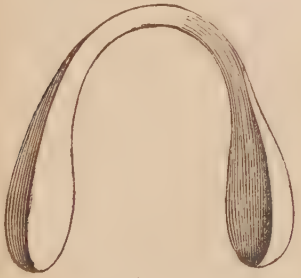


Fig. 1.

His instrument consisted of a thin, steel spring, four inches long, the points of which ended in wooden buttons, and the whole of it was covered with a thin layer of india-rubber. In introducing the instrument, the ends of it are approximated to each other as much as is required for its easy introduction into the vagina.

In applying it, it must be elevated in the direction of the lateral diameter of the vagina, while its convex portion is directed towards the anterior walls of the pelvis. The instrument thus bent is gently pushed upwards, so that its points take a position to the right and left side of the uterine neck, as high up as possible in the laquear vaginæ.

Although the instrument has been abandoned by the profession, owing to the fact that very few women can bear the pressure which it necessarily must exert, in order to sustain itself in the vagina, the elytromochlion of Kilian has been applied in some cases successfully, thus proving that the theory of its construction was based upon sound principles.

In 1853, Dr. Zwank, of Hamburg, published the description of his new *hysterophor*. It consists of two ovoid thin pieces of metal, covered with india-rubber, or of wood, connected on one end by a joint. In the neighbourhood of this joint, on the external surface of the wings, is a metallic pin, on each side two inches long, which can be screwed together at the lower end.

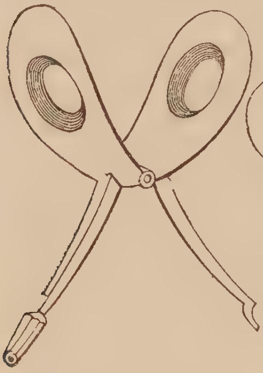


Fig. 2.

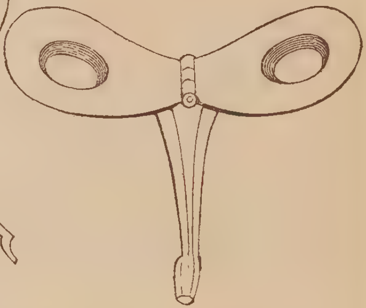


Fig. 3.

In applying the instrument, the wings are approached as much as possible (fig. 2), and introduced so that its convex portion is turned towards the os sacrum, and pushed upwards, as high as possible, towards the anterior portion of the laquear vaginæ, in front of the neck of the uterus. Afterwards the lower ends of the metallic handles are compressed, and fastened by the screw (fig. 3). In this position the instrument is retained by itself.

About the same time, Dr. Schilling, of Munich, invented quite a similar instrument to that of Zwank; the only difference being, that the movement of the wings is effected, and can be regulated by the screw at its lower end. The purpose of both instruments is, to gently expand the lateral portions, and sustain the superior wall of the vagina, thus preventing its inversion, and consequently, the falling of the womb.

Dr. Zwank's instrument was received enthusiastically by the profession in Germany. Such men as C. Mayer, Chiari, Braun, Scanzoni, Breslau, &c., thought it of sufficient importance, to publish their observations in favour of this instrument, and at the present time it has actually supplanted all of its kind.

What is the reason of this? Is it because the profession seized upon the instrument because it was a new invention? Is it because an instrument was wanted? or has it fulfilled what it claimed to do?

The question which we propose to consider, is whether this instrument has any advantage over others hitherto applied for the same purpose. It certainly has; because, 1. It is lighter. 2. It touches only a comparatively small circumference of the vagina, and scarcely any portion of the womb; thus preventing irritation and ulceration of the vagina, incarceration of the uterus, fluor albus, uneasy feelings. 3. It can easily be introduced and removed, easily brought to its proper place, easily cleaned by the patient herself. This is a combination of advantages, sought for in vain among the host of previously-invented pessaries. On the other hand, the hysterophors of Zwank and Schilling have some disadvantages, owing to the substance of which they are composed. The greatest number of them, as now in use, are covered with a coat of vulcanized india-rubber. The discharges of the vagina destroy it in a very short time. After this has been done, the metallic portions begin to rust and decay, thus irritating the vulva; the furrows of the screw at the lower end of the instrument begin to crust, or the screw, if turned too firmly, cannot be untwisted. Some patients have little dexterity, and do not know how to manage the screw at all. An illustration of these facts I am seeing daily, in the case of a lady belonging to the first class of society. She is the widow of a well-known physician of this city, and has suffered from prolapsus uteri ever since her first confinement, many years ago. The most thorough examination is unable to detect anything abnormal about her genital organs, except prolapsus uteri. She has been under the very best treatment of genital practitioners and uterine specialists. Everything has been resorted to, to effect a radical cure, and all kinds of pessaries used, but in vain. At length one of Zwank's pessaries was suggested. She has worn it now for a year, and is perfectly satisfied; the only drawback being the loss of the india-rubber coating, and the rusting of the metallic skeleton.

In order to avoid these inconveniences, Dr. Eulenburg, of Coblenz, modified Dr. Zwank's pessary, and described his instrument in a short thesis, in 1857. It is made entirely of boxwood, and its wings are a little differently shaped, viz.: they are slightly curved downwards at both ends, so that the lower side forms a concave surface. In consequence of this shape, the lateral branches closely adapt themselves to the inner surface of the ramus descendens ossium pubis; thus presenting a kind of hook, which gives a strong hold to the instrument when in the vagina. Both wings move in the centre part by two joints, thus leaving a hole in the middle, through which the secretions of the vagina are allowed to escape. Instead of the screw, Dr. Eulenburg perfected the opening and shutting of the wings, by means of an elastic india-rubber ring, which runs in a channel around the body of the hysterophor, immediately below the two joints.

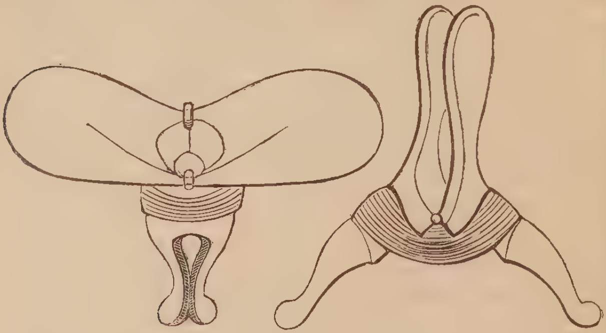


Fig. 4. Fig. 5.

By this contrivance, the introduction of the instrument is greatly simplified, and as it shuts on its own account, by the elasticity of the india-rubber ring, its application becomes very easy, thus requiring not the least ingenuity upon the patient's part (see figs. 4 and 5.) As every particle of metal is avoided (except the small pin, running through the joint), and as the boxwood resists more than any other substance the corrosive influence of the vaginal discharges, it is lighter, will keep longer, and will cause less irritation than the other instruments.

The author found *four* different sizes, fitting to the greatest number of cases, viz.: for the measure from side to side, 2¾″, 3″, 3¼″ and 3½″, and correspondingly the largest antero-posterior diameter of every wing, 1″ 3‴, for the two largest sizes, and for the following, 1″ 4‴ and 1″ 5‴.

The first application of the instrument ought to be performed by the physician himself, who has to choose the size required for every case. His judgment will be conducted by the sensation of the patient, after walking to and fro for awhile, and more so by the way in which the india-rubber ring contracts. If the extra-vaginal portion is not shut entirely, the instrument is too large, and has to be removed; if it shuts too quick, a larger one must be chosen. The following duties devolve upon the patient herself, viz., removing and cleaning it at bed-time, and re-adjusting it before getting up in the morning. This is performed by seizing the buttons at the lower end, and while separating them from each other, as much as possible, the other end of the instrument is to be gently introduced into the vagina till it cannot go any further; and (when left alone) now it shuts on its own account. The same way is followed in its extraction. Before its introduction, it ought to be well oiled. In order to render this pessary even more harmless, it is advisable to cover its branches with a kind of glove, made of soft deer-skin, which coat may be moistened with cod-liver oil before every application.

Of great importance is the breadth and direction of the pubic arch, because this is the chief guide for the selection of a pessary. It can be ascertained by introducing the second and third finger behind the arcus and expand both fingers till each of them touches one side of the arcus. The distance of the fingers thus obtained may guide our judgment in the choice of an instrument. As a general rule it may be stated, that a comparatively small instrument ought to be tried first, because it very often happens, that even the most extensive prolapsus is benefited by small instruments.

After the instrument has been closed, the patient must be questioned as to what her sensations are. If the instrument was too large, a singular kind of smarting is perceived and considerable uneasiness expressed. It is a good plan to have the patient walk around, in order to ascertain if the prolapsus will be perfectly retained by the instrument.

Even in cases where the perineum has been ruptured, our instrument has been used with perfect success; the only precaution to be taken, is the choice of a broad pessary.

On the second day after the application of the instrument, the patient must be seen again by her attending physician, because at this time generally certain symptoms occur, which originate from the presence of a foreign body in the vagina, and which prove, *if they are very intense*, that the instrument is too large.

The symptoms alluded to, are a chilly sensation, heat, headache, trembling, nausea, want of appetite, obstinate constipation. The instrument must be removed, and the vagina must be examined with the speculum, to see if a portion of it is inflamed or ulcerated, a condition always met with if the instrument chosen was too large. After the third day is over, chills and heat are very trifling, and disappear entirely some time afterwards.

If the instrument is borne after some days with no discomfort at all, the patient must be taught how to use it, and must repeat the manœuvre of adjusting and removing it several times in the presence of the physician.

At the time of the monthly courses, the patient had better have the instrument removed, provided she can keep quiet. At times it happens, that a portion of the anterior wall of the vagina falls beneath the pessary. In this case, a broader instrument must be chosen, or a small piece of plugged linen must be placed in the midst of the instrument, corresponding with the prolapsed portion, which is easily retained by this contrivance.

In recommending these instruments, and especially the latter one, to the consideration of the profession, I am sustained by the experience of our European brethren, who have used them with such general satisfaction, that scarcely any other form is now in use. Lately Dr. A. Mayer, of Berlin, has published a paper on the use of Zwank's pessary, wherein he reports to have successfully applied it in two hundred and thirty cases. For my own part, I avoid the use of pessaries as much as possible. But I have had under my care a number of cases, in which a pessary was the only means justifiable. I have tried a great variety of them, and have now come to the conclusion that Zwank's (or Eulenburg's) hysterophor answers better the requisites of a good pessary than any other.

I, therefore, ask practitioners to give it a fair trial. I do not mean to buy a hysterophor, and sell it to the next woman with prolapsus uteri, but after carefully selecting the case, in which nothing but a good pessary will give sufficient satisfaction, let the different sizes be tested, until the proper instrument is found.—*Contributions to Midwifery, p.* 38.

112.—ON VESICO-VAGINAL FISTULA—ILLUSTRATING A NEW MODE OF OPERATION.

By Isaac Baker Brown, Esq., F.R.C.S.

[At the commencement of his paper the author briefly relates several cases of this difficult lesion, six of which were successfully cured—making in all a sum of twenty-six cases—an amount of success greater than that published by any other surgeon. Mr. Brown then proceeds to offer the following practical observations and suggestions.]

I propose to speak—

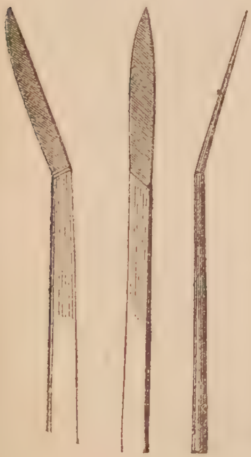
I. Of the position of the patient during the operation.
II. Of the various modes of operating.
III. Of the causes of the lesion.
IV. Of the causes of the failure.
V. Of the after-treatment.

I. *Of the position of the patient during the operation.*—Three positions have been recommended: on the hands and knees, as advised by Dr. Bozeman; in the semi-prone position, as suggested by Simms; in the lithotomy position, as recommended by myself. I am still of opinion that this latter position is best, both for the patient whilst under the use of chloroform, and for the surgeon, who can sit down quietly before the patient during the operation; whereas, on the hands and knees the patient cannot take chloroform, and the surgeon must stand in a very awkward posture during a long and tedious operation, which becomes exceedingly fatiguing. Of the semi-prone position I have had no experience, nor have I yet seen the necessity for its adoption. It must still be recollected that, whilst advocating the lithotomy position, there will be found some cases where, from peculiarity of the position of the lesion or of the female organs themselves, it may be advisable not to administer chloroform, and where the opening can be better seen and reached on the hands and knees than in the lithotomy position.

II. *The modes of operating.*—Four classes or modes may be mentioned: (*a*) by simple sutures as recommended by Hayward; (*b*) by metallic sutures as practised by Gossett in 1834, and more fully placed before the profession by Simms of New York; (*c*) metallic sutures guarded by clamps, as recommended by Simms, or by button, as suggested by Bozeman: (*d*) a plan which I now desire to recommend, namely, metallic sutures with a separate bar clamp to each suture. The suture may be either of silver wire or galvanized iron wire, as prepared by Cocker Brothers, of Sheffield, under the direction and suggestion of my friend Dr. Aveling. The first three plans have been fully elaborated by the gentlemen whom I have mentioned, and do not require any further observations here; I will therefore only dwell, and that briefly, on the fourth plan.

Having placed the patient in the lithotomy position, I commence paring the edges of the opening, using for this purpose the three knives represented in Fig. 1. As there has been some misapprehension about my claim as the originator of these knives, I may here state that they were designed by myself without communication with any person whatever, and publicly used and recommended long before my friend Dr. Bozeman came to this country. During one of the many discussions he and I had upon plastic surgery, I showed him these knives, as affording great facilities for denuding the edges of the fistula; when he, in his own quiet manner, opened his instrument-case and took out three knives precisely similar to mine, which were made in Montgomery (U.S.) from his own designs and under his own immediate eye. It will be thus seen that we both felt the necessity for, and had made, the same kind of instruments, without communication with each other.

Fig. 1.



The edges being pared, I now pass the needle as practised by Simpson, and then through it either the silver or galvanized iron suture. Having thus passed as many sutures as may be necessary, I take the two ends of the first suture, and pass them through the eyelet-hole of the simple bar clamp (Fig. 2), which, it will be seen, is slightly curved, and having a nipple projecting from its back. I then hold the two ends of the wire with my left hand, and seizing

Fig. 2.

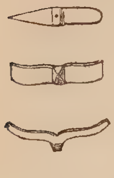


Fig. 3.

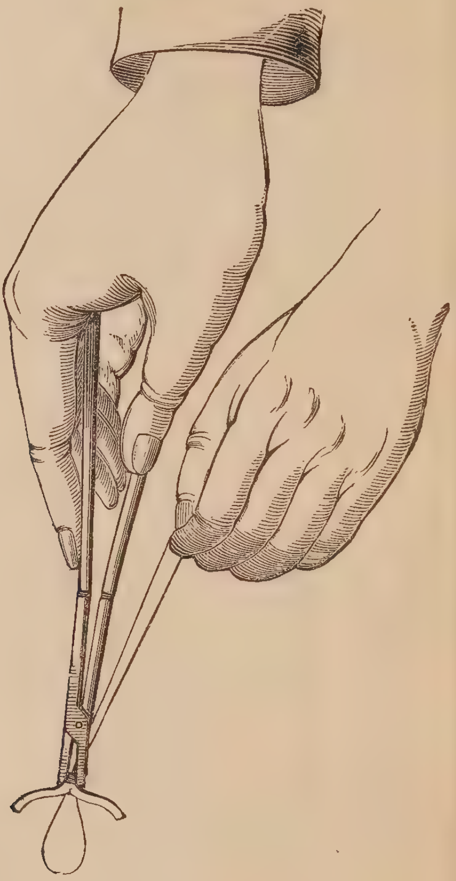
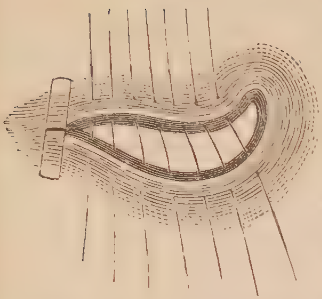


Fig. 4.



the nipple of the clamp with a pair of long forceps, pass it back until I see the edges of the fistula are in perfect contact. I then firmly squeeze the nipple (Fig. 3). This has the effect of partially straightening the clamp and securing the wires (Fig. 4). Each suture is treated in the same way until all are closed.

The advantages of this plan are manifold:—

1. Its celerity as compared with others.

2. The certainty that the edges are in perfect apposition all along the fistula.

3. That, however irregular the opening, you can follow its tortuosity without the slightest difficulty.

4. Should the opening be very large, the edges are kept perfectly together, whilst the vagina is not stretched out, as it would be under the large button. Each suture is also well guarded by the clamp, and there is no trouble in removing them. Simply the anterior side of each suture is cut, under the clamp, and then this, being seized by a pair of forceps, and slight traction made upon it, is easily withdrawn.

By this mode of operating, I have succeeded in completing the operation, in slight cases, within a quarter of an hour, and within three-quarters of an hour in the worst cases. Formerly, such cases took two or three hours; and I have heard Bozeman and Simpson say even a much longer time.

III. *The causes of the lesion.*—I here repeat that, in almost every case, this lesion has taken place from very protracted labour; and I cannot too strongly urge upon the profession to abolish the practice of waiting days and nights with the head pressing upon the bladder and soft parts generally.

IV. *Causes of failure after operation.*—It may arise from not carefully denuding the edges; from not introducing the needle deep enough through the tissues; from not being sufficiently careful in bringing together the edges of the fistula; from neglect in the position of the patient afterwards (this position should be upon the side); from neglecting to keep the bladder empty, either by the catheter being left in the bladder, or by its very frequent use. Occasionally the general health may be so much impaired as to predispose the patient to pyæmia, or to prevent healthy plastic matter being thrown out sufficiently soon to ensure union. It is necessary, therefore, to attend to the general health of the patient before operating. Another not unfrequent cause of failure arises from a certain indurated condition of the parts, which is induced by frequent operations, especially cauterization. The parts become almost cartilaginous, and are unable to throw out plastic matter. Such a case has come under my notice, where three fistulæ existed; two of these were easily cured, but the third has defied all attempts at present from this cause.

V. *The after-treatment.*—On the free use of opium and generous diet, and strict attention to the bladder, depend the success of the operation, however well performed in the first instance.—*Lancet, Dec.* 10, 1859, *p.* 581.

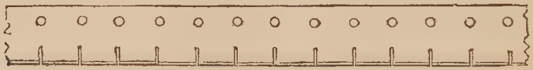
113.—A NEW PRINCIPLE OF TREATMENT, AND APPARATUS FOR VESICO-VAGINAL FISTULA.

By Robert Battey, M.D., Georgia, U. S.

[The principle of the treatment advocated by the writer is "direct pressure upon the approximated edges of the fistula," and the apparatus used to carry out this principle combines "both the compress and the splint;" and the author believes that by it cases of special difficulty can be treated with a certainty of success in the first operation, not hitherto attainable.]

The combined compress and splint shown (Fig. 1), is made of sheet lead, of such thickness as to be flexible and easily adapted to the vaginal curve, and firm enough to retain the form given to it. It is prepared in strips of a convenient length, and usually five-sixteenths of an inch in width, or wider if required in any special case. At a distance, of say one-sixteenth of an inch, from either edge of the strip, faint lines are drawn and crossed at intervals of three-sixteenths of an inch by similar lines, running transversely. The bar is now perforated with holes upon the one side, and notched with a very fine saw upon the other, as shown in the drawing. A piece of proper length is cut off, and smoothed with the knife, when required.

Fig. 1.

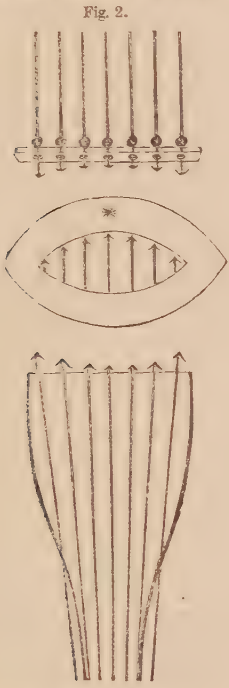


Full size.

Operation.—The edges are pared, and the sutures introduced as heretofore, inserting the needle usually three-eighths of an inch from the pared surface. When the tension upon the tissues is very great, it is desirable to take deeper hold, and increase a little the width of the splint. It is always desirable to bring out the needle at the edge of the vesical mucous membrane, including in the stitch as much as possible of connective tissue. As the wires are introduced they are passed through the holes in the splint, and secured by perforated shot, leaving an inch or more of free end, the splint being held up against the speculum, out of the way of the operator. Holding the splint in one hand, with the other tighten the wires, giving an equal tension to each, and draw down the splint upon the posterior lip of the fistula. If traction be now made upon the wires, it will be perceived that a remarkable power is had to drag down the vagina and uterus, giving easy access to the fistula; and also, if a thin strip of wood, bevelled to a dull edge, be placed under the suture wires (see Fig. 2), and pressed upwards, an equally marked power for carrying up the anterior lip and closing the fistulous opening.

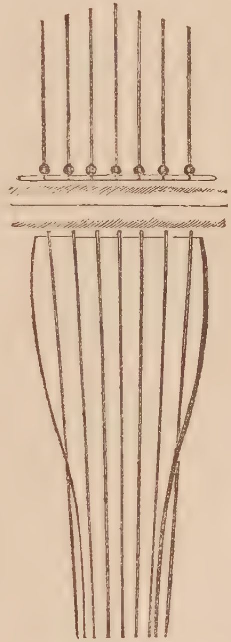
Fig. 3 exhibits the fistula closed and the splint turned up, giving a full view of the approximated edges, that the operator may assure himself that his work is as perfect as he desires. The splint is now

Fig. 2.



Half size. Shows the sutures in place; the splint turned upon its edge, and the wooden spatula applied to the proximal lip of the fistula.

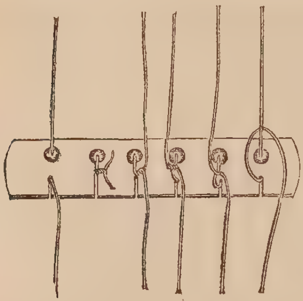
Fig. 3.



Half size. The opening has been closed by longitudinal traction, upon the wires, while counter-pressure is used by means of the wooden spatula.

turned down, and the wooden spatula confided to an assistant, who holds the parts in firm apposition while the operator secures the wires, as represented in Fig. 4. For this purpose a slender bit of wood, or the handle of an ordinary scalpel, with a small notch cut in the extremity, and a long pair of artery forceps, will be convenient and simple instruments. Placing the scalpel-handle under the wire, raise it into the notch in the splint, and push the wire on beyond the splint, as shown in suture No. 1 upon the right of the splint (Fig. 4); seize the distal end of the wire with the forwards, and bring it forwards around the loop and back again upon the other side, as shown in suture No. 2; still retaining the hold of the forceps, make traction upon both ends of the wire, and we have the two loops closely interlocked, as shown at suture No. 3; now seizing the wire at this point, clamp it firmly, make a secure twist (suture No. 4), and, cutting off the free ends, turn down the twist as shown in the finished suture No. 5. The manipulation is more simple than the description, and is executed with ease and rapidity after a little practice upon a wire run into the tablecloth. If there be much tension upon the tissues, and difficulty be encountered in retaining the wire in the splint notch until the twist can be made, the notch should be closed upon the wire by pinching the edge of the splint with strong forceps. This trouble may usually be avoided by having the notches sawed so as to fit the wire closely, when the friction will hold it quite securely.

Fig. 4.



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The advantages proposed to be gained by the new apparatus are—

1. The compress upon the approximated edges condenses the tissues, and, forming a water-tight joint, effectually prevents the leakage of urine, while no danger of strangulation is incurred; hence ensures the cure.

2. The compress directly hastens adhesive union, by bringing the surfaces with more certainty into firm and even contact.

3. The opportunity afforded of viewing the parts up to the point of final and satisfactory closure, is as complete as could be desired.

4. The power of the apparatus for bringing down the vagina, and for closing the opening, when there is great loss of substance and rigidity of tissues; and this with less danger of the wires cutting out.

5. The splint keeping the parts at rest, prevents the urine being drawn through between the sutures by capillary force; and offers a smooth, even surface upon which the cicatrix is to be formed.

6. The adaptation of this apparatus to recto-vaginal fistula, ruptured perineum, and also to hare-lip, is too obvious to require special comment.—*Lancet, Dec.* 31, 1859, *p.* 665.

114.—ON THE SURGICAL TREATMENT OF OVARIAN DROPSY.

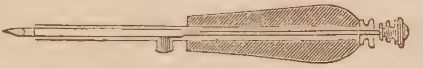
By J. Y. Simpson, M.D., F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[Respecting the position of the patient during the operation of paracentesis, Dr. Simpson commences by describing the position formerly adopted, viz., the patient sitting upright in a chair, with a sheet surrounding and supporting the abdomen, which was tightened at pleasure, by assistants standing behind the patient.]

I proposed and put in practice, several years ago, a new method of operating, in which the compress is entirely dispensed with, and the operation is rendered far more simple and easy at once for the patient, the practitioner, and the assistants. According to this plan the patient is not made to sit up at all, but is allowed to remain in bed in the horizontal posture; for she is less liable to syncope in the latter than in the former position; besides that she is by this means saved from all the fears and anxieties of what must always appear to her to be very formidable preparations. She is made simply to lie forward to the very front of the bed, and the protuberant abdomen is made to project to some extent beyond the edge of the bed. At the same time this part of the bed is, of course, to be protected by means of some folds of a sheet or towels. The trocar having been introduced into the cyst at the selected point, and with the usual precautions, the expulsion of the fluid is left to be effected at first by the elasticity of the abdominal walls, and afterwards by the force of the atmospheric pressure on the abdominal surface. As the evacuation goes on slowly and steadily, you can usually perceive the outlines of the secondary cysts—if such are present—gradually becoming more and more prominent; and in most cases we can perceive an elevated line passing from the anterior superior spine of the ilium towards the linea alba a little below the umbilicus, while on either side of this ridge the depression is usually most distinct. As the discharge proceeds, the cysts and abdominal walls covering it, become steadily and uniformly compressed and flattened, far more steadily and uniformly under atmospheric pressure than under the pressure of any kind of mechanical binder. When the cyst is nearly emptied, the evacuation of the last remaining portion of the fluid may be favoured by having the patient turned somewhat more prone, and sometimes perhaps—but not often—gently pressing the sac with the hands. But let me advise you to be careful not to manipulate ovarian cysts too much during their evacuation, lest in incautiously raising the hand after exerting some degree of compression, you give an opportunity for the atmospheric air to be sucked up through the canula into the interior of the cyst; for inflammation of the lining membrane of the cyst is liable to be set up as a consequence of its entrance. After the cyst has been as entirely evacuated as possible, place your finger on the orifice of the canula and cautiously withdraw the tube; the abdominal parietes which it traverses being compressed by the fingers and thumb as the instrument is withdrawn through them. A pad of lint is adapted over the wound, and strapped down by strips of adhesive plaster. These strips should be about an inch in breadth, but two or two and a half feet long; for when you use them of this length—placing them across the abdomen of the patient—they form at the same time the simplest and most efficient belt or bandage that you can apply, in order to effect some degree of compression, and thus prevent all possibility of the occurrence of syncope. I used at one time to place a binder around the patient after completing a tapping in this way; but the application of it causes her some degree of annoyance, and I am not aware that it fulfils any indication that may not be better and more simply answered by the long pieces of strapping of which I have spoken. They are in reality a series of small binders, more easy in their adaptation, and as efficacious in their action as the larger linen or flannel bandage.

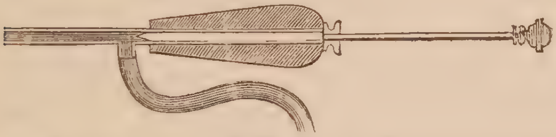
Instrument required for the Operation.—I have just a sentence or two more to add in regard to the kind of instrument with which the operation of tapping is to be performed, before I pass to the consideration of the other surgical means of treating cystic tumours of the ovary. Where the operation has already been repeatedly performed, and where you have reason to believe the surface of the cyst to have become adherent to the abdominal parietes a perforation may be made with a lancet or bistoury at once through the abdominal wall and the wall of the cyst; and through this opening the fluid may be allowed to drain away. But generally you will make use of the ordinary trocar and canula for the evacuation of ovarian cysts, and in regard to that instrument only two remarks occur to me as worthy of your special attention:—*First*, See that the trocar be sharp; for a blunt-pointed instrument causes the patient a degree of pain which may be almost entirely avoided by using an instrument in proper order, and which should be mitigated to a certain degree even in the use of a blunt one, by your making a preliminary incision through the skin with a lancet or bistoury. *Secondly*, Take care that the silver tube, or canula, be not too sharp at the point; for when this is the case, it may happen that the lining membrane of the cyst will be injured, and even a blood-vessel wounded; and hemorrhage into the interior of a newly-evacuated ovarian sac is a most untoward complication, and destroys in no small measure the patient's chances of recovery. I told you a minute ago that the entrance of air into the interior of the sac during its evacuation, was an accident which we ought to do all in our power to avoid and avert. In tapping the cavity of the pleura in cases of empyema or pleuritic effusions of any kind, the danger of allowing air to regurgitate into the space from which the fluid is escaping, is even greater than in abdominal tappings; and a variety of contrivances have been adopted with a view to prevent it. Canulæ with valves, or with stopcocks, or with a piece of soft tubing, or a fold of softened bladder or leather, attached to the extremity which would allow the fluid to pass outwards, but which collapsed under atmospheric pressure when the fluid ceased to flow; and variously modified instruments have also been used with the view of preventing the ingress of air into the cavity which is being emptied. Perhaps the best kind of instrument that you can use for this purpose is one first described by Mr. Charles Thompson in the 'Medical Times and Gazette' of March 27, 1858. (See 'Retrospect,' vol. xxxvii., p. 182). It consists of a

Fig. 1.



canula, to which the trocar fits tightly, like a piston, and which has a small projection at the outer or lower part. To this projection an elastic tube can be fastened with the view of carrying the fluid into a large tub or basin placed at the bedside; and if this tube be filled with water, and the end of it be immersed in that or any other liquid, it acts as a syphon after the trocar is drawn back, and promotes the more effectual evacuation of the cyst. See

Fig. 2.



Whatever be the kind of instrument you employ for emptying the cyst, you will, in many cases, find a difficulty in effecting its complete evacuation, which you cannot avert by any modification in the form of canula that you employ. This difficulty arises from the presence of small semi-solid masses which occur in greater or less abundance in many dropsical ovarian sacs, and which are apt to get into the tube, and plug it up, after the greater portion of the fluid has escaped, so that the removal of the latter portion of the fluidbecomes obstructed. These occluding masses are sometimes formed of lymphy exudations or other products of an inflammatory process. But they are occasionally found where no inflammatory attack seemed ever to have occurred, and in such cases they consist of the *debris* of the broken-down septa that once divided a single large cyst into several small ones, or of albuminous matters in a state of partial consolidation, or of blood-clots. Whenever, therefore, you go to tap an ovarian cyst, you ought to have some instrument by you with which you can free the canula of such obstructing masses, either by pushing them back into the cavity of the cyst, or, better still, pulling them outwards, so as to allow of the free escape of the remaining fluid. There is no instrument that you can use better adapted for this purpose than the ordinary uterine sound, which I should, therefore, advise you to take with you to every case of ovarian tapping. Besides making use of it for clearing the occluded canula, you can explore with it the interior of the cyst by passing it through the tube; and by this means you may gain some information as to the extent of the cavity, the number and size of the secondary cysts, and the presence of calcareous and other matters in the interior. For many years I was in the habit of using the uterine sound in this manner; and I have sometimes passed it between the surface of the tumour and the abdominal parietes, with the view of discovering the presence and extent of any existing adhesions. But I have seen dangerous symptoms result from the probing of dropsical cysts of the ovary, and I would advise you, therefore, to abstain from having recourse to it, except in cases where it is a matter of primary importance to gain certain information as to the condition of the interior; and even then, before having recourse to it, it would be well for you to pause and consider whether the information you are likely to receive by the process be of such value as to warrant you in subjecting the patient to the risks of inflammation attendant on the exploration. There is sometimes a farther source of difficulty in the complete evacuation of ovarian cysts, when a number of secondary cysts of large size are present; and in such cases the primary sac can only be emptied by making a perforation in the walls of the minor sacs, so as to allow them to collapse, and to be emptied of their contents also. This perforation you will be best able to make by means of a long sharp-pointed probe, such as the strong stylet of a large elastic catheter sharpened at the point. Passing this through the canula, and pressing the point of it steadily against the septum at its most yielding point, the instrument may easily be made to perforate it; and, if the contents of the subsidiary cyst be not too viscid, they will escape through the punctured orifice into the large cavity, whence they will escape through the canula. There is one great danger attendant on this proceeding, and that is the risk of injuring one of the blood-vessels that may be ramifying in the septum; and you must therefore avoid it as much as possible.

[With regard to the cases requiring the operation of injection of iodine, the writer observes:]

The occurrence of inflammation in the peritoneal covering of ovarian growths, and the formation of extensive adhesions between them and the adjacent surfaces, often form insuperable barriers to the operation of ovariotomy, but present no hindrance to the injection of iodine into the cavity of the cyst. But, on the other hand, I have in most cases been particularly careful to avoid injecting iodine into an ovarian cyst when I have noticed the contents to be mixed up with inflammatory products, although I have done so in a few cases where the application of the irritant to the chronically-inflamed surface seemed likely to produce no violent effect, but only a slightly alterative action; and I think it is always a safe rule to follow, not to have recourse to injections of any kind into an acutely inflamed ovarian cyst. For if the dangers of tapping are tenfold greater in cases where some degree of inflammation has been lighted up in the cyst previous to the operation, the risks, it appears to me, will be only aggravated by the application of any irritating substance to the already morbidly-disposed surface. Again, in cases where you have any great degree of difficulty in effecting a full and complete evacuation of a cyst from the commixture of a multitude of gelatinous semi-solid masses with its fluid contents, the injection of iodine is apt to be attended with this drawback—that it produces coagulation of the albuminous matters contained in the cyst, and thus forms a number of clotted masses, which cannot escape through the canula, and are not likely soon to be absorbed; but which are very liable to act the part of foreign bodies and lead to inflammation of the lining membrane. Probably some little chemical inquiry will betimes enable the obstetric practitioner to overcome this difficulty. Out of the body, the addition of a small quantity of aqua potassæ to the gelatinous fluid drawn from an ovarian sac will occasionally, if not always, make this fluid freely and perfectly mix with tincture of iodine; while before the addition of the alkali the iodine only mixed in points and patches with the ovarian fluid, and if injected by itself into a sac with these contents would have consequently been most inadequately and imperfectly applied to the lining membrane, provided, at any part, any of the secretion of the cyst remained. With the exceptions which I have stated, I believe the injection of iodine into the interior of evacuated ovarian cysts may, as a rule, be had recourse to with safety, with retardation of the disease in many cases and with permanent success and cure in some. The hope of benefit and of cure will vary with the character of the disease which is treated. In cases of multilocular cystic tumours, you can hardly hope by means of one single injection to produce a complete and lasting cure, for then you only empty and act upon the lining membrane of one out of a multitude of cysts, or at most of several cysts in communication with that one; and the utmost you can then usually expect is, that the chief cyst, or cysts, of the group will by this means become obliterated, and the progress of the disease be thus stayed for a time. In some rare cases, perhaps, the irritant injected into the cavity of one cyst may have the happy effect of inducing a certain amount of absorption in the walls of those around, so that in this way some of these other cysts may come also to be in some degree diminished. But I greatly doubt whether the iodine injected into one cyst ever has the effect of leading to the complete obliteration of the neighbouring sacs, unless it reaches their cavities; and this can only be done, in most cases, by having recourse to the separate tapping and injection of each particular cyst. Boinet places great faith in the treatment of multilocular ovarian tumours by injecting cyst after cyst; and there can be no doubt that a succession of cysts may be in this way cured by repeated injections. The cases, however, for which iodine injections seem to be best adapted are those where the tumour is unilocular, where the single cyst has been already tapped once or oftener, and still continues free from all trace of inflammatory action. In all such cases I believe iodine may be injected with the greatest possible degree of safety to the patient, and with the most hopeful prospects of a full and final cure. By means of iodine injections I have, as it appears to me, freed a number of patients from a disease which, if uninterfered with, or interfered with only in the way of a palliative paracentesis, must sooner or later have carried them off; and so far as I am able to judge, the cure has been complete and is likely to be permanent. Such seems to have been the case, for example, with a young woman who called at my house, along with a relative, the other day, and who was one of the earliest patients that I injected with tincture of iodine. She was tapped for the first time eight years ago, because of an ovarian tumour which had been growing for about a year before, and had attained such great size as to interfere with her breathing, and otherwise to render her uncomfortable. The fluid drawn off was perfectly clear; and in about six months it had grown as large as before, but then seemed to remain stationary. Six years ago, exactly two years after the first tapping, she was again tapped, and this time a quantity of tincture of iodine was thrown into the cyst. She had a slight degree of pain afterwards, which was subdued by a one-grain opium pill; and remained in bed for a fortnight. From that time to this she has suffered from no untoward symptom, and at present it is altogether impossible to feel any tumour whatever in the pelvis, either through the abdominal parietes or through the vagina, although when I saw her two years ago I could still distinguish an abnormal body there; and the contracted and atrophied cyst must doubtless still remain. How, then, is this operation to be accomplished?

Mode of Operating and Apparatus Required.—When you go to inject iodine into an ovarian cyst in any case where you have determined on having recourse to this operation, you require to take with you—1st, An ordinary trocar and canula, with which to puncture and evacuate the cyst; 2nd, A long glass tube, with a nozzle, capable of being adapted to, 3rd, A gum-elastic male catheter, with the opening at the end of the tube; 4th, A uterine sound; 5th, Ten or a dozen ounces of tincture of iodine, or of the liquor iodinei compositus; 6th, A piece of lint, and some long strips of sticking-plaster.

In performing the operation the patient is placed on her side at the front of the bed, as I have already described to you at some length, and the full and complete evacuation of the cyst is to be effected in the same manner, and with precisely the same precautions, as in an ordinary simple tapping. Having emptied the cyst, you place your finger upon the orifice of the canula, to prevent the possibility of the ingress of air, while an assistant fills the injecting apparatus with the fluid you have determined upon using. Perhaps the simplest and most convenient form of injecting apparatus is that which I now show you. (See Fig. 3.) It consists of a long glass tube (A), capable of holding eight or ten ounces of fluid, fitted into a pointed wooden box or nozzle (B). With this instrument you can effect your purpose perfectly well, placing the palm of the hand on the wide mouth of the tube, to prevent the action of the atmospheric pressure when you wish to restrain the flow of the liquid from the lower orifice, and removing the hand when you are prepared to allow the liquid to run out into the ovarian sac. But the use of such an instrument is attended with some inconveniences, that may be obviated by introducing between the nozzle and the long tube that is passed into the interior of the cyst an inch or two of india-rubber tubing (CC), which you can close at will, by means of a simple spring (D). So long as the spring is allowed to remain closed no fluid can escape from the tube; but the moment its hold is relaxed by the ends being compressed, the weight of the atmosphere suffices to drive out the fluid with the due degree of force; and should the flow appear to be interrupted, or should it cease altogether, from obstruction in the catheter, or some other cause, you can usually force it on easily enough by blowing into the tube with a moderate amount of force. To carry the fluid into the interior of the cyst you make use of the long elastic catheter, to which I have already referred. This is passed through the canula into the interior of the cavity; and before commencing the operation you must have made sure that the catheter and canula are of such respective dimensions that the former can be passed easily through the tube of the latter when it becomes necessary to inject. Before introducing the catheter into the cavity of the cyst, you must attach it to the point of the apparatus I have just described; and you must be careful to have it filled with the fluid by relaxing for a second or two the pressure of the spring on the intervening inch of india-rubber tubing. If you neglect this precaution, and introduce the catheter into the cyst before attaching it to the instrument and without filling it with the iodine, you can easily see that the liquid, ere it can enter the cavity of the cyst, must first

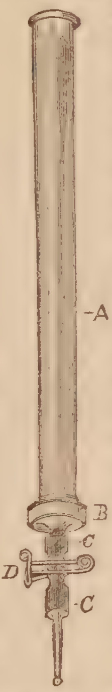


FIG. 3.—Apparatus used for injecting fluids into the interior of evacuated ovarian cysts.

drive before it the air that is contained in the catheter, and the operation becomes unnecessarily and unfortunately complicated by the presence of the air in the newly-emptied cyst. While the injection of the liquid has been going on, the patient is still lying on her side; but after enough has been thrown in, the catheter is removed, the finger is again placed upon the orifice of the canula, and the patient is made to turn on her back, and perhaps in other directions also, while the tumour is manipulated and kneaded with the free hand, with the view of insuring a more effectual application of the irritant to the whole surface of the lining membrane of the sac. When this has been done, the patient is again turned on her side, and the fluid is allowed to run out. Its escape may even sometimes be hastened by pressure cautiously applied ; for your object in injecting any preparation of iodine into the interior of an ovarian cyst is not to leave it there as a lasting irritant, but simply to paint all over its inner surface with a view of changing the secreting action of its lining membrane, and of setting up there the adhesive stage of inflammatory effusion. The injected fluid, then, having been all, as far as possible, again withdrawn, you proceed carefully to remove the canula, and afterwards to apply a pad of lint, and three or four straps of adhesive plaster, such as we saw to be necessary after an ordinary tapping. Such is the simplest, safest, and most satisfactory process that I know for introducing irritant injections into the interior of evacuated cysts of the ovary.

But there are still two questions in connexion with the operation that you will be ready to ask me, and which I must endeavour briefly to answer. What kind of fluid, or rather what preparation of iodine, is best adapted for ovarian injections? and, What amount of fluid may or ought at one time to be used?

Quality and Quantity of Iodine Required.—In reply to the former of these questions, the quality of the iodine solution, allow me to say, that the preparation which I have most frequently made use of for the cure of ovarian dropsy, is the ordinary tincture of iodine of the Edinburgh Pharmacopœia. This, as you are aware, is a very strong preparation, containing half-a-drachm of iodine in every ounce of rectified spirit. Fearing lest the alcohol in this preparation might produce its peculiarly intoxicating effect on the system, in some cases where there was a likelihood of the fluid being to some extent retained in the cavity, I have used the liquor iodinei compositus of the Edinburgh Pharmacopœia instead. This is a much less potent preparation, containing only eight grains of iodine in every ounce of the fluid, along with half-a-drachm of iodide of potassium. It has proved effectual, however, in a number of instances; and may be safely employed where you wish particularly to avoid all chance of the narcotic effects of the spirituous preparation. I have never seen these effects produced to any very alarming extent, even in cases where a large quantity of tincture of iodine has been used. At the worst the patient is incoherent, or lies in a drunken stupor for an hour or two, but she soon awakes from this condition as the exhalation and elimination of the poison goes on simultaneously from the lungs, the kidneys, and the skin. The alcohol is speedily dispelled; but for some days afterwards the odour of the iodine can still be perceived in the patient's pulmonary and cutaneous exhalations. You can also trace it chemically in the urine. As for the quantity of iodine to be used, that will, of course, vary according to the special case. When the cyst is of large dimensions, you can hardly hope to bring the irritant in contact with the whole of its lining membrane unless a considerable quantity be introduced. From eight to twelve ounces may be required for the larger class of cysts; and I have sometimes injected as much as fourteen ounces at once. For smaller cysts, from four to eight ounces may suffice; but there are probably very few cases where you will succeed in producing any appreciable effect by injecting less than six ounces. The quantity of fluid injected can easily be regulated either by having the glass tube graduated to ounces, or by keeping it in a bottle correspondingly marked and measured off.

There is still one point connected with the iodine treatment of ovarian dropsy, in regard to which you must permit me to make one or two remarks before I have done with this subject. I mean

The Mode in which Irritant Injections effect a Cure.—This is a subject that still requires investigation; and to the question, How does the injection of iodine cure ovarian dropsy? different authors have given different replies. Some say that it produces its effect by acting on the walls of the cyst in such a way as to destroy their power of secreting the fluid they habitually poured forth. Others, again, aver that iodine only cures by exciting such a degree of inflammation as leads to the complete obliteration of the cyst and the adhesion of the collapsed and contracted walls. Whatever views have been held, seem to have been adopted merely as a matter of opinion. There is no observation on record, so far as I know, to show what actually happens in cases where iodine has produced a cure. My own impression is, that the usual effect of the injection of iodine is complete obliteration of the injected sac by adhesive inflammation. I ground this opinion, *first*, On the analogous effects of the injection of iodine for the cure of hydrocele; and *secondly*, On the direct observation I had an opportunity of making in the case of a patient who had at one time been subjected to the injection of iodine for the cure of ovarian dropsy. The patient I refer to came to me, many years ago, suffering from a multilocular ovarian tumour, for which she had been tapped several times and in rapid succession, without any better result than the most temporary relief. I injected the most prominent cyst with tincture of iodine. She recovered well, and went home much reduced in size, and showing no appearance of any new development in the tumour. Long afterwards she returned to Edinburgh, wishing to be again operated on as before, as some of the secondary cysts had, after a time, taken on a more active growth, and the size of the tumour began to inconvenience her. She was fevered, however, by the fatigue of her journey, became rapidly exhausted, and died without anything being done surgically to the local disease. On making a post-mortem examination, we found the new cyst inflamed, and covered on the inner surface with patches of lymph ; and we could see besides what we supposed to be the old original cyst obliterated and filled up with plastic lymph, which was of a dark greenish colour, probably from the permanence of some of the iodine.

[On the subject of treating ovarian disease by the application of *electricity or galvanism*, Dr. Simpson observes :]

This means of treatment consists in causing a current of electricity, or galvanism, through the tumour, either by the application of the handles of the battery to the surface of the body or through the vagina; or by introducing, first of all, a number of acupuncture needles into the interior of the cyst, and causing the current to pass through them directly into the fluid. The effect, according to Jobert, would be the coagulation of the fluid, which, he avers, should take place in six or seven applications, and without determining any inflammatory action.

I have tried the effects of galvanism in many cases of ovarian dropsy, applying both through acupuncture needles, and simply to the surface; but the effect has usually been very slight, and I certainly never found any good result from it. On the contrary, in one patient in whom I made use of it, introducing some acupuncture needles into the interior of the cyst, and thus passing the galvanic current directly through the fluid, such a degree of inflammation was lighted up in the interior of the cyst, that she died in consequence of it.

[It is certainly only in rare and exceptional instances that *ovariotomy* is a justifiable operation. But the question is—In what instances ?]

First, It is never to be dreamt of under any circumstances or in any case where the patient is at the same time suffering under organic disease or derangement of the kidneys, lungs, heart, or any other organ; never, in short, unless the constitution of the patient is in other respects healthy and favourable. *Secondly*, It is never to be thought of in any cases of cystic disease of the ovary, when that disease is mixed up, as it sometimes is, with carcinomatous or encephaloid deposits in the tumour. The co-existence, or the fear of the co-existence, of cancer with the cystic ovarian degeneration, is a complete barrier to the idea of ovariotomy. *Thirdly*, It is not to be had recourse to—at all events never in the first instance—in cases where the disease consists of a single ovarian cyst, or of a mass of ovarian cysts, with one cyst of such a preponderating and predominant size, as to make the diseased mass resemble a unilocular dropsy of the ovary. *Fourthly*, It is not to be had recourse to in these cases, because we have good hopes of often controlling and sometimes curing the disease by milder and safer measures; as by 1, paracentesis, and allowing the fluid, if bland, to escape into the peritoneal cavity; 2, by tapping through the roof of the vagina, and, if necessary, establishing a fistula between the cyst and the vaginal canal; or 3, by obliterating the cyst or cysts by the injection of iodine. The only cases of ovarian dropsy fit for ovariotomy, are cases of the common compound or multilocular form of the disease, in which these preceding measures have failed to restrain the onward growth and progress of the disease towards the inevitable destruction and death of the patient. But all cases of compound or multilocular ovarian disease do not belong to this category. In a few, Nature, as we have seen, herself taps and relieves the patient from time to time. With these cases you would assuredly not intermeddle. In a few, the course of the malady is very slow, and seems occasionally—but very, very rarely—to become as it were arrested in its march. With these cases you would not interfere in the way of any surgical treatment whatever. In some instances of multilocular ovarian dropsy the fistulous tapping of the largest and predominating cysts, or the injection of them with iodine, stays or retards, and occasionally so arrests the further development of the malady, that no more serious or severe surgical measures are to be considered necessary. But, unfortunately, in a large number of multilocular ovarian dropsies, all these measures prove primarily or ultimately, totally ineffectual; palliative tappings come to be required more and more frequently; and it becomes a matter of almost mathematical certainty that the poor patient will ere long sink under their repetition, or under a continuance of the disease. Still, when the practitioner is driven to bay in this manner, the remaining cases are not by any means all of them cases for ovariotomy. Already in many such firm adhesions exist between the surface of the ovarian tumour and the contiguous surfaces of the péritoneum, that if the operation were begun it could not be finished. It is this circumstance that has, perhaps more than most others that I alluded to in my last lecture, prevented ovariotomy from being adopted by some surgeons into the class of legitimate operations. Out of the 292 cases of ovariotomy collected by Fock, in only 200 instances was the operation completed, with the result of 111 recoveries to 89 deaths. But out of his collection of 292 cases, there were no less than 92 instances in which the operation, after it was begun, was not completed; and in 71 out of these 92 the cause of failure was the extent or strength of the old inflammatory adhesions of the sac to the contiguous peritoneum. Those practitioners that are in the habit of performing ovariotomy do not regard now a considerable amount or extent of adhesions as an insurmountable barrier to its completion; but where the adhesions are very great, the proper plan of surgical proceeding—if any surgical proceeding beside tapping is adopted—is, as I have already tried to show you, the establishment of a permanent or fistulous communication between the largest cyst or cysts and the cutaneous surface. The cases, then, fitted for the operation of ovariotomy are specially, if not entirely, those in which the tumour is multilocular; in which tapping and other milder surgical measures have failed to stay the progress of the disease, or are inapplicable from the great number and small size of the component cysts; in which the disease is evidently going on steadily, and at no remote period to the destruction of the patient; and yet the tumour feels entirely or comparatively free from morbid adhesions, and the general constitution of the patient is such as to present no counter-indication to the performance of this formidable operation. Under such a painful combination of circumstances, the question comes to be, Are we to recommend our patient to linger on for a few months or weeks longer, and submit to the usual progress and fatal termination of the disease, and to die without any surgical proceeding at all, or are we to counsel her rather to submit by ovariotomy to an operation that certainly may very probably inflict speedy death, but which, on the other hand, if it prove successful, gives her all the best chances of renovated health and prolonged life? I must here emphatically repeat what I have stated to you before, that in this, as in other capital and dangerous operations for chronic disease, the patient herself, or her friends, ought as much, or more than the physician, to decide the question of surgical interference or non-interference.

Supposing that you had made up your mind as to the possibility of removing the ovarian tumour, and that the propriety of the operation had been determined, it only remains to consider how the operation is to be effected. Let me, therefore, as briefly as possible, explain to you

The Mode of Performing the Operation.—1. *Preliminary Measures.* —Before exposing your patient to the risks of this, as of any other serious operation, you are, of course, bound, in the first instance, to prepare your patient, as far as possible, by attempting to raise the standard of health, by means of tonics, and to diminish in some degree the chances of inflammation, by putting her for a time on antiphlogistic diet and regimen. Some have recommended that the heat of the room in which the patient is to be operated on should be raised some degrees above the average temperature, under the idea that the cold air might prove irritating to the exposed peritoneum. But such a precaution seems hardly to be necessary; for there is no direct proof that the peritoneum is more likely to be inflamed in consequence of being exposed to the atmosphere at a low temperature, than it is when exposed to heated air. If the room be heated, care must be taken to prevent the air from getting too dry, by impregnating it with the vapour of water. When the operation used first to be performed, indeed, the greatest care was taken to have heated cloths in readiness to apply to the peritoneal covering of the intestines, when by chance they were protruded; but those who are in the habit of operating frequently apparently lose all such great fears in regard to the free exposure and rough handling of the peritoneum. Even where inflammation has existed, and adhesions have been formed between the surface of the tumour and the abdominal wall and viscera, these adhesions have in numerous cases now been freely torn up, and the peritoneum, roughly sponged and wiped, without giving rise to any such dangerous consequences as the former prejudices of the professional mind would have led us to expect.

2. *Instruments Required.*—The instruments necessary for operation are but few, and easily enumerated; all that are required being a scalpel or bistoury for making the incision and dividing the neck of the tumour; a trocar and canula, for evacuating any large cyst; a clamp, for securing the pedicle, such as I show you (Fig. 4), or perhaps some acupressure-needles, instead—which would be of use also for arresting the hemorrhage from any vessels that might be divided in large adhesions—or you may prefer to use the artery forceps and ligature, or torsion; and, finally, a needle and iron wire will be required for sewing up the wound.

Fig. 4.

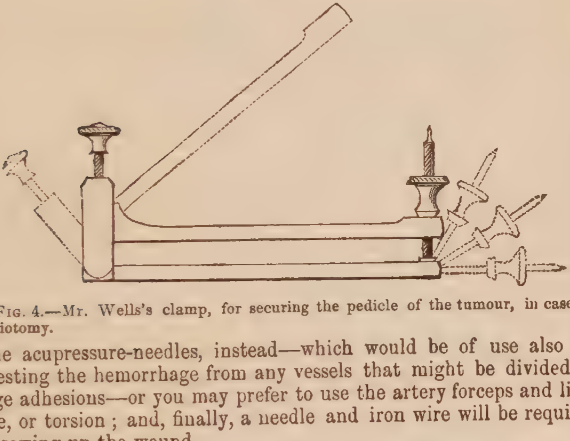


Fig. 4.—Mr. Wells's clamp, for securing the pedicle of the tumour, in cases of ovariotomy.

3. *Position of the Patient.*—It has been the common custom to place the patient near the foot of a bed, the thighs separated, and the feet supported by footstools, the surgeon sitting or standing between the legs of the patient. But there are obvious disadvantages in such a position. The surgeon is awkwardly placed, both for operating, and for seeing and securing bleeding vessels. So are the assistants. It is also a fatiguing position for a patient—one in which it is difficult to keep her covered; and one in which fainting is more likely to occur than in the recumbent position. The latter is much to be preferred. In hospitals, the ordinary operating-table answers very well; but in private practice you are not likely to find in many houses the sort of

bed or couch which is altogether suitable. You want a narrow table, which can be readily moved; and Mr. Spencer Wells tells me that he finds the table which he generally uses answer the purpose remarkably well. It is a couch which he keeps in his consulting-room, and it is so made that, by unscrewing the legs and folding up the frame, it can be readily taken to a patient's house, as the old lying-in chair still is in some parts of Europe. The pillows are so arranged that they can be fixed at either end, so that they cannot slip off; and there is a moveable tray beneath to hold the instruments. The table is brought to an angle of about 45° to the window, so that the light falls well both for the operator and assistants. The gentleman who administers chloroform stands at the head of the bed. The operator is placed on the right side of the patient. An assistant may stand on the left of the operator, and two or three on the opposite side of the patient, all seeing well. The contents of the cyst are carried by the tube trocar to the pail below. The patient is carefully protected from cold by blankets folded round the feet and legs, and others round the chest, her own night-dress being pushed up under her arms, to keep it dry, so that there may be no necessity for changing it when she is lifted from the couch to her warm, dry bed.

4. *The Operation.*—There are several stages in the operation, each requiring our special attention. We begin with,

a. The Incision.—The operation is begun by making an incision with an ordinary scalpel or bistoury through the abdominal walls in the line of the linea alba, from the region of the umbilicus, downwards towards the pubes. Cases of ovariotomy were at one time divided into two classes, called respectively the operation by the short and by the long incision, according as this incision was small in extent, and begun below the level of the umbilicus, or was much longer, and stretched considerably above the umbilicus; and the relative dangers or advantages of the two modes of operation were at one time warmly discussed. It was very generally believed, at first, that the smaller the opening in the abdominal parietes, the safer the operation would be; but further experience has shown that the greater liability to peritonitis and displacement of the bowels which was supposed to attend the operation by the long incision, is more than counterbalanced by the greater liability to laceration of some of the tissues and escape of the cystic contents into the peritoneal cavity, and by the greater difficulties which present themselves in the course of the operation by the short incision. So that now most operators seem to prefer to open the abdomen freely, so as to be able more easily to separate adhesions and bring out the tumour, believing, at the same time, that the risk of peritonitis being set up is almost, if not altogether, as great when an opening is made, barely sufficient to allow of the tumour being dragged through it. The incision is not made at once through all the thickness of the wall, but only down to the outer surface of the peritoneum, which is first punctured and then divided in the rest of its extent by means of a probe-pointed bistoury or a herniotomy knife.

b. Evacuation of Cyst or Cysts.—The tumour will be found pressing forward in the opening, and tending to bulge through it; and if there be one or more cysts of large size, it is well at once to tap them, as by this means the bulk of the mass to be removed will be much diminished. In this case, perhaps, there is no better instrument for withdrawing the fluid than the trocar and canula I have already shown and described to you, with an india-rubber tube attached to it to carry away the fluid to a receptacle on the floor, without exposing the patient's person and clothes to the discomfort of being soaked by it in running off. Here, however, such an effectual emptying of the cyst need not be insisted on, and therefore the instrument I refer to may be used for the sake of the decided advantages which it offers in other respects.

c. Separation of Adhesions.—The tumour thus diminished in size is to be drawn out through the opening in the abdominal walls; and to be able to do so you will require sometimes to separate any adhesions the tumour has contracted with the neighbouring organs. These adhesions can usually be most safely separated with the fingers or the handle of the scalpel. Sometimes, however, they are too firm and of old standing, and require the use of the blade for their division. Occasionally, a vessel bleeds, or the inner surface of the abdominal wall on the severance of some well-established adhesion. In such a case you must endeavour at once to arrest the hemorrhage, and I believe you will succeed in doing so with ease and success by means of an acupressure-needle entered from the cutaneous surface, and passed through the thickness of the abdominal wall till its point finally emerges in the cutaneous surface, at a distance of one and a-half or two inches from its point of entrance. The head and point of the needle will thus remain exposed on the surface of the abdomen while the vessel on the inner surface will be compressed between the resistant tissues of the wall and the short bridge of the needle that lies exposed towards the peritoneal cavity.

d. The tumour, then, having been brought out through the wound, you have next to proceed to cut across the pedicle, and use some means to prevent the flow of blood from its divided blood-vessels. And here, perhaps, we meet with the question of greatest importance in connection with the operation. The earlier operators used to tie the pedicle tightly at its narrowest part with a piece of whip-cord, and then to divide it close to the tumour. But as it sometimes happened that the cord lost its hold and slipped off the cut extremity, thus giving rise to hemorrhage internally, this means was speedily given up. Then, for a time, operators adopted the plan of passing stout threads or pieces of thin cord through the neck of the tumour at one or more points, and strangulating it in the same way as we do when operating for the cure of internal piles, or an erectile tumour of the skin, by means of the ligature. By this means it was found possible to control the hemorrhage most effectually, and when the threads had been duly tightened, their ends were brought out through the lower angle of the wound in the abdominal parietes. But as these threads acted the part of irritant substances in the peritoneal cavity, and lighted up in many cases inflammation of this membrane; and as the gangrenous and ulcerating stump of the pedicle within the grasp of the ligatures was also a further source of irritation and of danger, all who have thought much upon the subject have felt anxious that some other means should be devised by which these dangers might be avoided. Dr. Marion Sims has proposed to substitute metallic ligatures for cord and other organic ligatures for tying and securing the ovarian pedicle. He did so under the hope that the metallic would prove less irritating than the organic thread, and might be left lodged *in situ.* But the pedicle on the free side of the ligature would be sure to die and slough, if the ligature were drawn tight, whatever was its composition. Mr. Spencer Wells, some time ago, suggested the division of the pedicle by the *écraseur*—a plan which has been followed successfully by Drs. Atlee and Pope in America in some cases, but in others it has not afforded that complete immunity against hemorrhage which is necessary for the safety of the operation. In London they now make use in most cases of a clamp (see Fig. 5) such as I have already shown you, which is screwed firmly across the pedicle, and after the tumour has been cut away, the stump, tightly embraced by the clamp is brought out, and made to lie exposed on the surface of the abdomen transversely to the line of incision. You might not be able at once to conceive how the comparatively short stump of an ovarian tumour could thus be brought and kept on the surface of

Fig. 5.

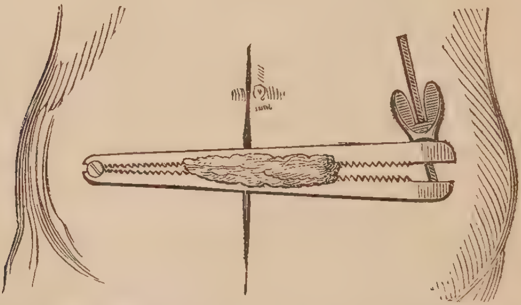


FIG. 5.—Sketch showing the manner in which the pedicle is secured with the clamp, and the position in which the clamp and pedicle are kept when the operation is completed. (Wells.)

the abdomen, without producing an injurious straining on the organs and vessels with which it is connected. But if you will only bear in mind that the abdominal walls, so long and so much distended during the growth of the ovarian tumour, must now remain collapsed and yielding after that mass has been removed, like the loose and flaccid walls of the abdomen after parturition, the difficulty will disappear. That a certain degree of dragging, however, is still kept up—particularly in cases where the pedicle or stump happens, as sometimes occurs, to be very short—we can hardly doubt; and, although the clamp has in a number of cases been found to answer well, yet its use is attended with certain dangers and drawbacks, that render it still desirable to find out some simpler means of arresting the flow of blood from the cut ends of the vessels in the divided pedicle. I believe by the application of acupressure we may yet be able to check the hemorrhage effectually and more safely than by any of the methods hitherto employed for the purpose. A needle, or needles, such as I show you might, before the pedicle is divided by the knife or *écraseur*, be passed through the abdominal wall, and through one margin of the pedicle from without; and after being made to cross nearly the whole breadth of the pedicle, it could be made to pass through its other margin, and then outwards again through the abdominal wall, so as to compress the pedicle between the exposed portion of the needle and the abdominal parietes with a degree of force sufficient to arrest the flow of blood from its injured vessels. The pedicle being thus pinned against the abdominal parietes in the iliac fossa, there would be no dragging or displacement of the pelvic organs, and no danger of incarceration of the bowel, as has just happened in one case in London, where the pedicle was fixed to the wound; and the risks attendant on the sloughing and ulceration of the stump would be avoided, for the acupressure-needle could be removed after fifty or sixty hours, or perhaps even less—after the vessels had become occluded, but before the pressure exerted by the needle had been kept up for such a length of time as to produce that ulceration which we know to be the necessary result of too long-continued pressure on any portion of the animal frame. Should the resistance offered by the abdominal walls not be found sufficient to enable the single acupressure-needle to produce efficient compression, a second or third could be introduced, either parallel with the first, or embracing only a limited portion of the pedicle, or crossing the others as they enter and emerge from the pedicle. If we can succeed in restraining the bleeding from the divided neck of the tumour by some such means as this, we shall be able to do away with some of the greatest sources of danger in connection with the operation, and so to perform it with better hopes of ultimate success.

Closing the Wound.—Having secured the pedicle of the tumour, all you have now to do is to wipe the peritoneum thoroughly clean, and then to proceed to close up the external wound. This you may do either by means of needles like those used in the operation for hare-lip, or, what is equally efficacious, by means of simple metallic stitches —either of silver or of iron. If you are operating on a wealthy patient, or on the happy inmate of some overflowing wealthy institution, you may, perhaps, prefer to use the suture of silver, which certainly has this superiority over the iron thread, that it is more expensive. But usually, I believe, you will rather employ the iron wire, which is at once cheaper and stronger, and as efficacious and manageable. A variety of needles have already been invented for stitching up wounds with metallic threads; but, however ingenious, they are all quite superfluous, for the instrument that you will find the most convenient and the best for sewing wounds with wire-threads is the common needle in a fixed handle, generally spoken of as a "tumour needle," or "pile needle," and constantly used for passing ligatures through the base of hemorrhoidal and erectile tumours which we desire to strangulate. By means of such a needle, with a considerable curve, you can easily take a deep hold of the abdominal wall; and in closing up wounds in the abdomen it is advisable always to pass the suture deeply, so as to keep the divided margins of peritoneum in perfect contact. The needle is passed unarmed through both sides of the wound—close down upon the peritoneum, but without wounding it. I should say, however, that Mr. Spencer Wells is strongly in favour of carrying the sutures through the peritoneum, as he thinks the cavity is then more effectually protected from the pus and secretions of the wound. When the point has protruded through the further side, the end of the wire is passed to the distance of half-an-inch or so through the eye of the needle, which, as you know, is situated near its point; and when the needle thus armed is withdrawn, it leaves in its track the wire-suture, which may then be twisted or tied across the line of incision, and which need not be removed till the whole wound has completely united.

5. *The After-treatment.*—When the operation has been completed, the patient is put back to bed, and kept thoroughly clean, quiet, and undisturbed. No special kind of dressing is required for the wound; and, indeed, you will find it best to keep it altogether undressed, and merely protected by means of an arched frame placed over the patient's body. Only in cases where the clamp is made use of and the sloughing stump of the pedicle is brought to the surface of the abdomen it is sometimes found necessary to apply a poultice of charcoal, or some other disinfectant, to counteract the disagreeable odour arising from the putrescent discharges. I would further enjoin on you the propriety of keeping the patient under the influence of opium, with the double view of dispelling the pain and depression which almost necessarily succeed such an operation, and of checking, in some degree, the action of the bowels,—an indication of paramount importance, when we remember how essential it is to the ultimate success of the operation to have all the parts concerned kept for some days at most per-

fect rest. At the same time the patient may be allowed to partake freely of ice, which is useful at once in counteracting the sickening effects of the operation, and of the opium, and in moderating the feverishness to which she is inclined. Where there is a tendency to sinking, the patient must have an allowance of wine or brandy. Peritonitis not unfrequently springs up; and, of course, you must be prepared to meet it, and to treat it by all appropriate measures. We may sometimes expect also to meet with other

Results of the Operation.—I have already spoken at sufficient length of the degree of mortality attendant on the operation; but I have said nothing as to the ordinary sources of danger. These are, however, so obvious, that I need not do more than name them now. Thus, for example, many patients die exhausted, from the shock of the operation. Sometimes hemorrhage occurs from some of the pelvic vessels, which it is found impossible to control. A case occurred lately in London where hemorrhage nearly proved fatal, which was found to be proceeding from an injured vein in the pelvis; and I have known a patient dying some hours after the operation, where there can be little doubt that the fatal issue was mainly due to the immense quantity of blood lost from a wound in some of these veins, which it was impossible to trace out and close. Fatal hemorrhage, of a secondary nature, sometimes, though rarely, occurs from the vessels in the stump, and carries off the patient. Perhaps the most frequent cause of death in connexion with the operation is peritonitis. Occasionally, patients survive all the more immediate perils of the operation, to be wasted away and finally to succumb under the effects of prolonged discharges and chronic inflammations. But sometimes, also, we have the happiness of seeing patients survive all the dangers of the operation—both immediate and more remote—restored to the active duties of life and the full enjoyment of perfect health, and some of them even giving birth to families afterwards.—*Med. Times and Gazette, Jan.* 7, *Feb.* 4, *and March* 24, 1860, *pp.* 1, 107, 283.

115.—OVARIOTOMY.

(Cases under the care of SPENCER WELLS, Esq., Surgeon to the Samaritan Hospital.)

[The first case was in a single woman, aged 29. The tumour had existed altogether a year, and had been tapped five weeks before admission, with but little diminution in size. The patient was pale, weak, and emaciated, but the tongue clean and appetite good. The circumference, at the umbilicus, was thirty-six inches. It was decided to perform ovariotomy without delay.]

On Oct. 12, chloroform having been administered, Mr. Wells removed the tumour by the small incision. He divided the abdominal parietes over the linea alba to the extent of about four inches midway between umbilicus and symphisis pubis, thus making an opening just large enough to admit his hand. Passing this all over the surface of the cyst, he broke down some extensive adhesions to the parietes, and then emptied several of the larger cysts through a large trocar, drawing them out one after the other as they were emptied. Some portions of semi-solid matter, and aggregations of small cysts were drawn out, and a short pedicle secured by a clamp, about an inch from the right side of the uterus. The left ovary was found to be healthy. The abdominal and pelvic cavities were carefully cleaned by sponging, two small vessels were secured by ligature, the clamp fixed on the surface of the abdomen, and the wound accurately closed by deep and superficial wire sutures; four of the former being passed through the whole thickness of the parietes, including the peritoneum. The tumour consisted of a few large and a great number of small cysts filled with a viscid fluid, the whole weighing about forty-five pounds.

Progress after Operation.—It is unnecessary to follow the daily notes of this case, as for the first week it was one of gradual recovery. There was so little pain that no medicine was given either by mouth or rectum for the first four days. The pulse remained feeble, ranging from 80 to 100, and perspiration was very profuse. There was very little vomiting. Hot linseed poultices were kept to the abdomen, brandy and soda water, port wine, beef-tea, and the brandy and egg mixture of the pharmacopœia, were given at short intervals. On two or three occasions, on the fifth and sixth days, when there was a little pain, twenty minims of laudanum in an ounce of water were thrown into the rectum. The bowels acted spontaneously on the fourth day. The clamp was removed on the same day, and the deep sutures. She went on remarkably well until the tenth day, when the wound was perfectly united, with the exception of a small depression at the lower part, where a small slough on the stump of the peduncle interposed. On wiping away this piece of slough very free hemorrhage took place suddenly, and several ounces of blood were lost during ineffectual attempts to find the bleeding vessel. Mr. Wells then applied a saturated solution of the perchloride of iron in glycerine, which instantly arrested the hemorrhage. She did not appear worse for the bleeding, but did not improve during the next three days, and on the fourth day the bleeding recurred. The clot formed by the perchloride had been loosened, and the hemorrhage was rather alarming. On carefully wiping away all clot, Mr. Wells then discovered that the bleeding vessel was not in the peduncle, but was a small artery in the parietes which had apparently been wounded in passing the lowest suture. This was tied, and no further bleeding took place, but there was for some days rather a free discharge from the lower part of the wound of fetid sero-sanguinolent grumous fluid and broken-down clots of blood. She remained weak for some time and the bed-sore which she had before the operation increased and caused her a good deal of pain, but she gradually gained strength and left the hospital in a very good state of health on the 7th inst. Dr. Jackson has since written to say that she bore the long railway journey to Barnsley very well, and was "in the enjoyment of capital health and spirits."

[The second case was a lady's maid, single, aged 27. The tumour first appeared four years ago, remaining, however, but small for a year and a half, then commencing to increase. She was tapped for the first time in August, 1858, and up to the time of operation on Dec. 6, 1859, was tapped seven times more. The cyst was evidently multilocular, and a large solid mass could be detected on the left side. At the operation,]

Mr. Priestley administered chloroform. The hospital staff were present, and Drs. Sims and Westmacott. Mr. Wells commenced by making an incision over the linea alba, midway between umbilicus and symphisis pubis, just large enough to admit his hand, intending to do no more if the adhesions proved to be very firm ; but as he found the greater portion of the surface of the cysts nearly free, and some adhesions at the anterior border of the solid portion yield readily before the hand, he determined to proceed, and emptied both cysts through a very large trocar. They were then drawn forward, and the wound enlarged up to the umbilicus. Some firm adhesions of the omentum to the upper and back part of the solid portion were then separated, and two or three small cysts burst during this part of the operation. The whole of the cysts and tumour were then drawn out of the abdominal cavity. The mass was attached to the left side of the uterus by a large broad peduncle, and in addition to this a thick vascular band of adhesion connected the lower part of the solid portion with the abdominal wall behind the sigmoid flexure of the colon. This band was divided by the ecraseur. The peduncle was secured by a clamp, and the tumour removed. Two vessels which were bleeding freely, far back on the abdominal wall, having been opened in separating adhesions, were then secured by ligature, the pelvis and lumbar fossæ carefully sponged out, and the wound united by deep and superficial sutures of iron wire. A pad of lint and a flannel belt were then put on, and the patient placed in a warm bed. She remained very low for about an hour, when she rallied and began to complain of pain in the abdomen. Half a drachm of laudanum was thrown into the rectum, and the injection was repeated in an hour, but only twenty minims were then thrown up. Some brandy and soda water were given, and a little port wine. She was very comfortable in the evening. The pulse about 90. There had been some vomiting, but it was not very troublesome. The skin was warm and moist, and she was in good spirits. She passed a very tolerable night ; had two injections of twenty minims of laudanum when the pain became troublesome ; a moderate quantity of urine had been twice removed by the catheter. She had slept pretty well, and up to 10 a.m. on the day after operation seemed to be going on satisfactorily.

She then began to sink rapidly, felt cold and faint, and the pulse became imperceptible, notwithstanding the free use of stimulants both by the mouth and rectum. She died twenty-three hours after operation.

The tumour consistsed of two very large cysts, which contained upwards of forty pounds of fluid, and of a semi-solid mass of small cysts, which weighed eleven pounds and a-half. The whole removed, therefore, weighed about fifty-three pounds.

The abdomen was examined on the following day. The wound was found to be united on its peritoneal aspect by recent lymph, with the exception of the spots where the peduncle passed below, and the ligatures on the vessels above. One loop of small intestine adhered by soft, recently effused lymph to the wound for the extent of about an inch. The portion of omentum which had been separated from the cyst was slightly congested. There were between one and two pints of bloody serum in the peritoneal cavity, but not a morsel of clot. There were signs of peritonitis about the broad ligament on the left side, and on the parietes near the wound, and on two or three coils of intestine which lay near it; but there were no marks of general peritonitis. The ligatures did not appear to have set up peritonitis in their track. The uterus was normal. The right ovary was about the size of a large walnut, and nodulated on its surface. The peduncle, consisting of Fallopian tube and broad and round ligaments, was securely fixed by the clamp outside the wound.

Remarks.—This was one of those desperate cases where the surgeon who looks to his own reputation as an operator whose results are numerically successful—or, less selfishly, to the credit of surgery in general, or of ovariotomy in particular—would certainly refuse to operate. In a case where the probabilities are generally against success an operator is not only likely to lose by a comparison with the results of others who only operate in favourable cases—but he may bring discredit on an operation, strengthen the belief in its excessive mortality, and deter other surgeons from recommending it, or patients from submitting to it, where the conditions are hopeful. And it becomes a moral question of no small magnitude how far these considerations should prevail against the simple question,—"What is my duty to this patient ?" It appears to us that if once the surgeon is convinced that he ought not to allow any patient to go on through a life of suffering to an inevitable death, when there is a possibility that an operation may restore that patient to health, it is his duty to cast aside all other considerations, and do his best for the poor creature who has confided in him. Then, of course, come the important practical questions,—Is the disease necessarily fatal ? Is an operation necessarily fatal ? Is there a moderately fair chance of recovery ? Does the patient fully understand the risk ? Is suffering so great and life so irksome, that she is anxious to be relieved even at so great a risk. ? All these questions were most anxiously considered in the above case, and, although the result has been unsuccessful, the mother of the patient, and those who knew her best and know what death by the natural progress of ovarian disease is, have a certain melancholy satisfaction in feeling that nothing has been left undone which might have been done, and that some weeks of hopeless, lingering suffering have been spared.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 605.

116.—HYSTERIC TYMPANITIS, OR PHANTOM TUMOUR OF VERY LARGE SIZE.

(Case under the care of Drs. Ramskill and Jones, Metropolitan Free Hospital.)

[The following notes of a very interesting case still under observation at the Metropolitan Free Hospital, were originally made in September 1857. The patient is a girl, aged 19, unmarried, and of peculiar and morbid temperament.]

"Her abdomen presents pretty exactly the appearance of a seven months' pregnancy, and her father even went to the length of turning her out of his house on the supposition that such was her condition. She applied to several medical men, and very different opinions were expressed. On admission, it was found that the abdomen, although round and full, was of clear percussion note in every part, that the umbilicus was depressed instead of protruding, and that no tumour whatever could be felt. On vaginal examination the uterus was found small, and its cervix of due length. Respecting the absence of any tumour in the abdomen, some caution must be exercised in giving an opinion as she invariably holds the muscle so tense and board-like, that pressure cannot be made with any satisfactory results. At times the abdomen will appear to be larger on one side than on the other, and the external contour as a very large ovarian cyst is very exactly simulated, but then there always remains the clear percussion note, which is inconsistent with such a supposition."

The girl has since been in several other hospitals; and she states that at one she was strongly urged to submit to the operation of paracentesis. Latterly she has again been attending at the Metropolitan Free, under the care of Dr. Jones. The prominence of her abdomen is now such that any one not having witnessed the trial of chloroform, might well be excused for entertaining the utmost scepticism as to the possibility of its disappearance. It exactly resembles that of a patient at the full period of pregnancy. The eye of a careful observer may, however, detect some differences between the girl's bearing, and that of a pregnant or dropsical person. Her back is more arched, and her gait has less of the swinging motion from side to side—"waddling"— a difference no doubt due to the fact that she has no real weight to carry. About two weeks ago, in order again to make a thorough and satisfactory examination of the state of things, she was put under the full influence of chloroform.

"The abdomen rises evenly in all directions, and is most prominent about two inches below the level of the umbilicus. A brown streak extends from the umbilicus to the pubes. The area of clear tympanitic resonance extends over the whole front of the abdomen. The measurement over the most prominent parts is thirty-three inches and one-third. Posteriorly both loins are dull on superficial percussion, the left side being entirely dull, even on the deepest percussion, while on the right a forcible stroke elicits a decided degree of resonance. As unconsciousness was supervening, under the influence of chloroform, the abdomen at one flattened, until not the slightest appearance of tumour existed. It was soft in all parts, and the promontory of the sacrum could very easily be felt. No escape of flatus was noticed.

"The abdomen now measures only thirty-one and a half inches. Examined per vaginam the cervix uteri was found to be long, and in every way healthy; the uterus was freely moveable. Fæces are felt in the rectum, and a mass the size of a small egg, to the left side, about which some difference of opinion existed as to whether it was fæcal or otherwise. As the influence of chloroform was passing off, the abdomen quickly resumed its original state, and in a few minutes after, when she had regained consciousness, it was nearly as prominent as ever. She took the chloroform remarkably kindly, and recovered from its effects very quickly."

One of the interesting features in this case is its long continuance. All forms of anti-hysteric remedies have been exhausted upon it without the slightest result. Marriage is, indeed, in all probability, the only remedy which can be expected to influence a condition which partakes more of moral than of physical derangement. Several similar cases have recently been under our notice, in one of which the patient has been told by several surgeons that she is the subject of ovarian dropsy. In this instance, also, as in the above, the patient is unmarried, and the same peculiarities of aspect are exhibited as have been just described. The value of the exhibition of chloroform as a means of diagnosis can be scarcely overrated. The cases are those to which mistakes in diagnosis are of the utmost importance, since they would inevitably lead either to unfounded imputation on the patient's chastity, or to the belief in the existence of incurable disease, or to the attempted performance of most dangerous operations. The records of surgery contain the narratives of not a few of such, in which either paracentesis has been performed, or the still more fatal mistake com mitted of laying open the abdomen, in order to extirpate an ovarian cyst. Against such errors the exhibition of chloroform affords entire security, and when any doubt offers about a case, it ought never to be omitted. As insensibility is induced, the board-like condition of the abdominal parietes, which had previously baffled all attempts to ascertain the exact state of the viscera. disappears, the diaphragm rises, the belly flattens out laterally, the anterior prominence disappears, and the hand can be passed back till it touches the sacral promontory.

Those desirous of further information on this subject, may refer to Dr. O'Ferrall's paper; to a series of cases published in this journal some years ago: and to a note by Dr. Priestley on Hysteric Tympanitis, in our first volume for 1858.—*Med. Times and Gazette, Dec.* 10, 1859, *p.* 579.

117.—DEODORISATION IN CARCINOMA UTERI.

By Thomas Skinner, M.D., Liverpool.

[After a few preparatory remarks, the author states in succession the means tried by him for the purpose of obviating the extremely offensive odour of the vaginal discharges in cases of Carcinoma Uteri. We give his observations upon the two remedies found to be the most effectual.]

Condy's Disinfecting Fluid (purple). This fluid, which is, I believe, a concentrated solution of the permanganate of potass, was used in the proportion of two drachms to a pint of water as a vaginal injection three times a day. The proportions were rapidly increased up to one fluid-ounce to a pint of water. The result was in every way satisfactory. A gentleman digitally examined the patient with me a few days after it had been used, and the smell was very much lessened, as also the amount of the discharge. (At this period of my observations, a common pewter vaginal syringe was used, and I cannot but think that this accounts for the success being incomplete.) One great advantage in the use of the permanganate is, that it has no smell of its own, and that, being soluble, it may be used either in the form of a medicated pessary, or in that of an injection. Its lovely *mauve* colour renders it both fashionable and pleasing to our patients. From my experience of its use, I can confidently recommend it in the proportion of one fluid-ounce to a pint of water, as a deodorant in cancer of the uterus; a cupful to be used as an injection thrice daily, and the strength to be increased if found necessary.

Tar-Water. Aqua Picis Liquidæ, D. While reading a report of the discussion on deodorisation between MM. Renault and Velpeau lately, I observed that it was merely a matter of economy and of olfactory preference, which made the difference between the use of coal and vegetable tar in the manufacture of the disinfecting powder; and, as I was much inclined to believe that the tar, or one or more of its active principles, is the essential ingredient, I determined to try the effect of the injection of tar-water. I was strengthened in this idea by having read the eulogiums passed upon it by Bishop Berkeley and others, and in the pages of the Medical Commentaries, alluded to by Dr. Mackenzie at the meeting of the Medical Society of London, on October 24th.

My patient has now used it undiluted for a month or more, and is likely to continue its use, as its beneficial effects have far surpassed my most sanguine expectations. The discharge has much decreased, and is for the present at a minimum. The foul smell entirely disappeared on the first day she used the tar-water as an injection; and, in its stead, the apartment, the bed, and the patient, have a mild and really refreshing aroma of a shipbuilder's yard in the distance. The patient and her friends have become so accustomed to this slight, and rather agreeable odour, that they are not now sensible of its existence; and they, as well as myself, can bear witness that none of the many measures adopted for the purpose of deodorising the discharge, have been so eminently successful as the simple injection of tar-water.

Formula for Tar-Water. Tar-water was at one time officinal in the Dublin Pharmacopœia; but, in the last edition, the formula has been omitted:—"Aqua Picis Liquidæ, D. Tar, lb. j; water, cong. j; mix, stirring with a stick for a quarter of an hour; as soon as the tar has subsided, strain the liquor, and keep it in well closed jars." (Neligan's Materia Medica, 3rd ed., p. 374.) Pereira gives a different formula:—"Tar, Oij; water, cong. j (wine measure):" otherwise the same as Neligan. My patient has always used the formula recommended by Neligan.

Properties. Tar-water ought to be perfectly clear, and of a pale sherry or amber colour. It has a bitter, resinous, and considerably acid taste; and a slightly creasotish, but rather agreeable empyreumatic odour. It reddens litmus-paper, and has a density of about 1·005. "The volatile oil in tar-water is partly held in solution by acetic acid, which, as is well known, dissolves creasote. It" (tar-water) "consists of water holding in solution acetic acid and pyrogenous oil and resin." (Pereira's *Materia Medica*, p. 1205.) The late Professor Royle says, that tar-water contains "creasote and other matters in solution." (Manual of Materia Medica, p. 647.)

Mode of Administration. By the aid of Higginson's invaluable syringe, a tepid solution of soap and water is first to be injected; and, immediately thereafter, a breakfast-cupful or more of undiluted tar-water. This should be done twice a day, if the patient's strength will admit: hitherto, a more frequent use of it has not been required. It is necessary that the tar-water should come into contact with the the ulcerated surfaces: therefore, the recumbent posture is preferable, and a long vaginal nozzle to the syringe is of advantage. If there is an intelligent, handy nurse in attendance, and the ulcerated cavity is extensive, and its lips lobulated and lapping over each other, a piece of lint saturated in tar-water, with a piece of tape or thread attached to it, may be inserted within the cavity, or the point of the nozzle may be directed into the ulcerated cavity before injecting.

Lastly, as important in itself, and as bearing upon the action of many of the substances alluded to, I would briefly call to remembrance the well known properties of

Creasote, which literally signifies flesh-preserver (from κρέας, flesh, and σώζω, I preserve), is the most important of the products obtained from wood-tar by Reichenbach, in 1830. It dissolves several salts, particularly the *acetates*, the acetate of silver excepted, which it reduces. It is itself completely soluble in acetic acid, wood naphtha, and alcohol, in most volatile oils, in the alkaline solutions, and in from eighty to one hundred parts of water. Like the chlorides of iron, mercury, zinc, and other caustics, it is a powerful coagulator of albumen; but, unlike these agents, the insoluble compound which it forms with the tissues *resists putrefaction.* In fact, it is the best known antiseptic; consequently, it is a powerful deodorant. Finlay Dun says: "It is believed to have been the essential agent used in embalming the Egyptian mummies. It is extensively employed in preparing various dried meats, and might be used for preparing subjects for dissection, by dissolving it in acetic acid, and injecting the solution into the veins. . . . Externally, it acts beneficially as a stimulant and astringent, an antiputrescent and deodoriser." (Veterinary Materia Medica, pp. 199, 200.) Of the use of creasote as a deodorant in carcinoma uteri, I believe that Dr. Copland was among the first who employed it. (Cyclopædia of Practical Medicine, vol. iii, p. 1283.)

In connexion with the deodorising property of creasote, it is interesting to observe that *carbolic acid*, which is undoubtedly the essential ingredient in Smith and McDougall's patent disinfecting powder, is supposed by M. Laurent to be really identical with *creasote.* (Royle's Materia Medica, p. 712.) The late Professor Gregory arrived at the same conclusion as M. Laurent, and about the same time. Professor Gregory stated to the Royal Society of Edinburgh: "If these two compounds be not identical, they are at least very closely connected, and in all probability contain the same radical. It is possible that creasote may be a definite compound of carbolic acid with some allied body. At all events, *it is very remarkable that these two compounds, described as different, should agree in density, taste, smell, antiseptic property, power of combining with bases, power of dissolving resins, indigo, etc., and finally, in composition,* although probably perfectly pure creasote has not yet been analysed." (Edinburgh Monthly Journal, 1845, p. 811.) Cooley, speaking of commercial creasote, says:—"The fluid sold in the shops for creasote is a mixture of creasote, picamar, and the light oil of tar; in many cases, it is little else than impure carbolic acid, with scarcely a trace of creasote." (Cyclopædia of Practical Receipts, 3rd ed., 1856, p. 665.)

It would appear from the above statements, that, to the best of our knowledge, the disinfecting agent in even Smith and McDougall's powder is none other than creasote, or its analogue carbolic acid; and, as carbolic acid is the acid portion of the volatile oil which is obtained by the distillation of *coal* tar (*Royle*, p. 712), I make no doubt that creasote or carbolic acid is the active ingredient in the powder and paste of MM. Corne and Demeaux, as prepared from *coal* tar. Creasote is doubtless the active ingredient in the powder and paste prepared from *wood* tar. There can also be no doubt that creasote is the active ingredient in woodsoot *(fuligo ligni)* and its preparations, which are so highly extolled by the Dublin and continental physicians. (*Neligan*, 3rd ed., p 35; *Pereira*, 3rd ed., foot-note to page 2014. See also an interesting case of the successful external application of the *decoctum fuliginis*, in the case of a severe burn, by Dr. Ebers of Bordeaux, in the *Edinburgh Monthly Journal* for 1841, p. 755.) I feel much inclined to try the *decoctum fuliginis* of the Dublin physicians as a deodorant in *carcinoma uteri*, or the *tinctura fuliginis*, sufficiently diluted with water. For the best account of creasote, I beg to refer to Dr. Cormack's Harveian prize essay for 1836.

The *modus operandi* of creasote, or any of the substances spoken of in this paper as antiseptics or deodorants, I have purposely avoided, as I consider the subject of too speculative a nature to serve any practical purpose.—*British Med. Journal, Dec.* 3, 1859, *p.* 980.

118.—*Cylindrical Pencils of Tannin in Certain Affections of the Uterus.*—This form of application, pointed out by Dr. Becquerel, seems likely to be of service in the treatment of lesions affecting the cavities of the neck and body of the uterus. In particular, in the fungous conditions of their mucous membranes, with consecutive hemorrhages, the tannin pencils might be advantageously substituted for the intra-uterine injections, which are not always free from danger. Dr. Becquerel's formula is—

℞. Tannin, 4 parts; gum tragacanth, 1 part; bread-crumb, q. s. to give the proper consistence.

These pencils are 5 millimetres in diameter, and 3 centimetres long. To use them, the neck of the uterus is exposed by means of the speculum ; a pencil of tannin is introduced by means of the forceps into the os tincæ, and is then pushed into the uterine cavity, and secured there by means of a plug of lint soaked with a concentrated solution of tannin. Once in position, the pencil softens and dissolves, and modifies the tissues with which it is in contact. At the end of twelve hours, the plug of lint is withdrawn by means of a thread attached to it. Every three or four days a new pencil is introduced in the same manner; and after a month of this treatment, the fungous state of the mucous membrane progressively disappears, and the hemorrhages are arrested.—*Bull. de Thérap.*—*Edinburgh Med. Journal, Jan.* 1860, *p.* 664.

119.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By John Erichsen, Esq., Surgeon to University College Hospital.

In many cases of tumour of the breast, as well as elsewhere, it is quite possible to pronounce positively, and at once for good or for evil. If the patient be middle aged, if the tumour be hard and adherent to the skin, and more particularly if it be ulcerated; if there be a chain of indurated glands extending into the axilla; if the countenance be sallow, with glistening eyes, and the frame emaciated, the surgeon can seldom be wrong in at once deciding the tumour to be cancer; on the other hand, when the patient is young, the tumour hard but moveable, the constitution delicate but good, the surgeon may require time to perfect his diagnosis. This uncertainty with regard to diagnosis of the tumours of the breast is an inherent element of these diseases, and may even continue after the growth has been removed from the mamma. In many cases surgeons will be in doubt about its nature, and this doubt is not cleared away until the tumour is subjected to careful microscopical investigation. So great is this difficulty, that the most skilful surgeons will occasionally err. It is true that the progress of the disease and the state of the constitution of the patient will eventually throw light on the nature of the disease; but there is nothing pathognomonic in the early stage of the growth of these tumours, and it is at this period that the great difficulty in their diagnosis arises.

In the early stages of all tumours of the breast, the surgeon is at last compelled to form his judgment of their nature by the manipulation of the mass. You can readily understand that, however skilled the sense of touch, however much he may possess the *tactus eruditus*, he may easily be deceived. For instance, if you were to put half a dozen apples under a napkin, and by simple manipulation you had to determine which one was a Ribstone pippin, which a golden one, you might in a few cases be right, but in many you would certainly err This is precisely what happens in tumours of the breast, though the history of the case will often reduce to a certainty the opinion we had formed of a tumour from its feel, and which otherwise would have remained obscure and unsettled. There is no class of surgical diseases in which errors of diagnosis are so likely to occur as in that of diseases of the breast. The most skilful surgeons may err, and have frequently done so; and, indeed, you would gain more instruction from an account, if it were possible to write such a one, of the errors of surgeons in these matters, than by reading any account of the diagnosis of these diseases. It is in a great measure in consequence of, and by taking advantage of these errors, that the cancer-curing quacks, who, whether lay, clerical, or medical, flourish in all large towns, derive much of their reputation with the public. They have this advantage over medical men. Suppose the "cancer-curer," as he calls himself, fail in effecting a cure, he is then no worse off than the regular practitioner, by whom the disease had previously been pronounced to be incurable. But if a medical man makes an error, and diagnoses cancer when in reality the tumour is a benign one, the patient, knowing that medicine affords her no relief, and that surgery only offers the palliative measure of extirpation, applies to a "cancer-curer"; he uses his caustics, and destroys a tumour, which, being non-malignant, though erroneously believed to be cancer, does not return. Such a case is ever after looked upon by her friends as an incontestable cure of a cancer, which had been pronounced to be incurable by medical men. Hence it is very necessary not to hazard a positive opinion in doubtful cases, or at all events not to throw one out on the gloomy side.

So far as the breast is concerned, for all purposes of surgical diagnosis, its tumours resolve themselves into two classes; first, the simple; second, the malignant. The simple non-malignant tumours again resolve themselves into three classes; first, *abscesses;* second, *cystic tumours;* third, *solid tumours* of various kinds. These are the distinctions you may establish for the purposes of diagnosis. Now let us examine these different classes a little in detail.

First, with regard to the diagnosis of abscess. Now, there are four kinds of abscesses of the female breast; three of these are not difficult to distinguish as abscess; the fourth often is so. The first is the ordinary subcutaneous abscess between the skin and gland, generally near the areola; it is accompanied by the ordinary symptoms of phlegmonous and suppurative inflammation—heat, pain, and throbbing. There is usually no great difficulty in diagnosing this variety; it is often met with at the age of puberty, in consequence of the increased action set up about this time. It occurs alike in single or married females, and usually without any uterine complications. In the diagnosis of this subcutaneous abscess, error will, however, sometimes creep in. For instance, a gentleman of considerable professional skill and acquirement, one day brought a patient to my house with "a tumour of the breast", which he said would require removal. On examining the bosom, I found one of these subcutaneous abscesses, with a good deal of œdema around it. I retired with my friend into another room, told him my opinion, and proposed to open it there and then. "For God's sake," said he, "don't do that. I have said it was a 'tumour', and I shall get into a scrape if you open it." I told him not to make himself uneasy about that, for I would soon settle matters to his and to his patient's satisfaction. I accordingly told the lady that I was glad to say that an operation would not be necessary, as we should be able to bring the tumour "to a head", and so disperse it. I accordingly ordered some resin cerate to be applied; and in a week the abscess had burst, and there was an end of the "tumour", to the great satisfaction of all concerned.

Two other of these varieties of abscess are of an acute and phlegmonous character. The first, that in the substance of the mammary gland; the second, that which forms behind the gland, in the cellular tissue lying between it and the pectoral muscle. These abscesses generally occur during lactation; and, by coupling the acute phlegmonous symptoms with the presence of lactation, there can be very little difficulty in distinguishing them.

The fourth form is that most frequently confounded with solid tumour. It is the chronic, cold, encysted abscess of the breast. This is a sort of abscess which forms deeply in the mammary gland, slowly, and without any acute symptoms—usually, indeed, without any local symptoms at all. It is hard, deeply seated, and tolerably well circumscribed. This encysted abscess of the breast has been especially described by Sir A. Cooper and by Velpeau. Both these eminent surgeons record cases of it which have been mistaken and operated on for cancer. A story has been told me by a gentleman who was present, of a surgeon now dead, but one of the most eminent of the many great surgeons that France has produced, who, after lecturing to his class just before operating on a case of tumour of the breast, and explaining, with that minuteness of detail and that lucidity of arrangement which the French surgeons possess in so high a degree, the reasons why the tumour he was about to remove could by no possibility be any but a scirrhus, made his first incision, when a gush of pus took place, and the supposed scirrhus was resolved into an encysted abscess, deeply seated under the mammary gland. Now this accident, happening as it did to one of the most eminent of his day, may occur to others of less professional experience; it has, to my knowledge, occurred six or eight times. But it may generally be avoided by attention to the history of the case, and especially to four points of diagnosis. These points, to which I would direct your special attention, are the following:—

1. Encysted abscess is invariably preceded by some uterine disturbance. You will find that the patient has lately been in the family way, and has either been delivered or has miscarried; or, if not pregnant, has suffered from some other uterine derangement—in most cases, however, of a parturient nature—a few weeks or months previously.

2. You generally find that the tumour, if an abscess, though hard and defined, is not distinctly circumscribed.

3. You almost invariably find—and this is a most important point—œdema of the subcutaneous cellular tissue, near the outer or under border of the gland.

4. There is also usually a spot on the surface of the tumour softer and more tender than the rest, possessing, perhaps, a certain degree of elasticity.

Now, by attention to these four points—namely, pre-existing or concomitant uterine derangement, hard but non-circumscribed character of the swelling, the œdema around or beyond, and the soft tender point—you can rarely go wrong in making your diagnosis. In illustration of this, I will mention a case which occurred here some time ago. A woman, thirty-six years of age, miscarried the sixth or eighth week of her pregnancy. Eight months after this, she applied to the hospital for relief for a tumour of the breast. On examination, I found a circumscribed painless swelling deeply seated in or under the mammary gland. There had been no pain or throbbing in it, and there was no redness over it. At the lower border, there was a spot which felt doubtfully elastic. I made an exploratory puncture into this, and let out about an ounce and a half of creamy pus. A tent was then introduced; and the induration of the walls, which were excessively thick, gradually disappeared, as the wound healed by granulation.

Besides these chronic abscesses of the breast itself, you may get another class of cases; that is, abscess in the neighbourhood of this organ, quite unconnected with the mammary gland. If there is any doubt in your minds, after examining such a case, always put in an exploring needle or trocar; its puncture can do no harm. and it will always clear up the true nature of a doubtful case.—*British Med. Journal, March* 31, 1860, *p.* 239.

120.—*Excoriated Nipples.*—Dr. Pierce observes that he has seen the stramonium ointment, as well as the whole list of astringent and stimulating preparations used; but not one has been so universally followed by good results as that made after the following prescription. ℞. Acid tannic. gr. xx., glycerinæ, alcohol. aa, ℥j. M.—*Boston Medical Journal.—Med. Times and Gazette, Feb.* 25, 1860, *p.* 197.

121.—ON THE DIATHETIC DISEASES OF CHILDHOOD.

By William Jenner, M.D., Physician to University College Hospital, and to the Hospital for Sick Children.

[The great diathetic diseases of childhood are four, viz., rickets, tuberculosis, scrofula, and syphilis. All are liable to produce striking and peculiar deviations from the healthy structure of particular organs and tissues. The following is a brief enumeration of the leading features of a typical case of each of these four general conditions:]

Tuberculosis.—Nervous system highly developed; mind and body active; figure slim; adipose tissue small in quantity; organization generally delicate; skin thin; complexion clear; superficial veins distinct; blush ready; eyes bright; pupils large; eyelashes long; hair silken; face oval, good-looking; ends of long bones small, shafts thin and rigid; limbs straight. Children the subjects of tuberculosis usually cut their teeth, run alone, and talk early.

Leading Pathological Tendencies.—Fatty degeneration of liver and kidneys; deposits or formations of tubercle, and their consequences; inflammation of the serous membranes.

Scrofulosis.—Temperament phlegmatic; mind and body lethargic; figure heavy; skin thick and opaque; complexion dull, pasty-looking; upper-lip and alæ of nose thick; nostrils expanded; face plain; lymphatic glands percepitible to touch; abdomen full; ends of the long bones rather large; shafts thick.

Leading Pathological Tendencies.—Inflammation of the mucous membranes of a peculiar kind; so-called strumous ophthalmia; inflammation of the tarsi; catarrhal inflammations of the mucous membrane of the nose, pharynx, bronchi, stomach, and intestines; inflammation and suppuration of the lymphatic glands on trifling irritation; obstinate diseases of the skin; caries of bone.

Rickets.—Mental capacity and powers small; muscular force deficient; mind and body inactive: figure short; closure of the fontanelles retarded; face small, but broad; skin opaque; often set with downy hairs. Children the subjects of rickets are late in cutting their teeth, in running alone, and in talking, and their teeth drop early from their sockets.

Leading Pathological Tendencies.—Softening of the bones; enlargement of the ends of the long bones; thickening of the flat bones, and deformities consequent on these conditions of the bones; so-called hypertrophy of the white matter of the brain; chronic hydrocephalus; pulmonary collapse; laryngismus stridulus; convulsions; albuminoid infiltrations of the liver, spleen, lymphatic glands, &c.

Syphilis.—Adipose tissue small in quantity; muscles flabby; cutis rough—deficient in contractility; complexion muddy.

Leading Pathological Tendencies.—Suppurative inflammation of the mucous membrane of the nose; ulceration of the mucous membranes of the nose and of the lips, mouth, throat, and anus; falling of the hair; eruptions on the skin of peculiar character; induration of the liver; suppuration of the thymus, lungs, &c.

There are pathologists of high repute who regard rickets, scrofulosis, and tuberculosis to be mere modifications of the same disease. I and others hold them to be distinct affections. The whole difficulty of the question lies in the difficulty of determining what is necessary to constitute identical diseases. With reference to some diseases the grounds of separation are broad and unequivocal. Scarlet fever and measles are distinct diseases, because their specific cause is different. Syphilis and tuberculosis are undoubtedly not identical, because syphilis owes its origin to a specific cause, and tuberculosis does not. Rickets, tuberculosis, and scrofulosis are due, it is said, to mal-nutrition, and therefore it is urged they are essentially identical; but it is manifest that the term mal-nutrition is at once very vague and very comprehensive. All diseased action by which unhealthy structure is formed in the place of healthy may be called mal-nutrition. Pus is the result of mal-nutrition, cancer is the result of mal-nutrition, tubercle is the result of mal-nutrition,—and yet the purulent, cancerous, and tuberculous diatheses cannot be held to be identical.

I hold rickets, tuberculosis, and scrofulosis to be distinct diseases in the sense in which tuberculosis and cancer are distinct diseases, and for the following reasons:—

1. Because the general condition in each is perfectly different from that in the other.

2. Because the pathological tendencies of those who manifest these different general conditions are different.

3. Because we rarely see the pathological tendencies of the one manifested by those, the subject of the others, *e.g.* rickets is absolutely unfavourable to tuberculisation — tuberculisation to strumous ophthalmia.

4. Because, tuberculosis being unquestionably hereditary, we do not find the children of phthisical parents specially prone to rickets or to scrofulosis.

5. Because, although we often find several members of the same family, the subjects of rickets, of tuberculosis, or of scrofulosis, it is comparatively rare for members of the same family to be the subjects of more than one of these diatheses.

6. Because rickets is not hereditary in the sense in which tuberculosis is hereditary.

7. Because the course, prognosis, and treatment of each of these diatheses are different.—*Med. Times and Gazette, March* 17, 1860, *p.* 259.

122.—ON RICKETS.

By William Jenner, M.D., &c.

[Though generally met with among the children of the poor, in large towns, rickets is occasionally seen in those very circumstances least likely to produce it, viz., in the children of the wealthy, living in good country air.]

Rickets is a general, or diathetic disease, manifested after it has existed a longer or shorter time, by certain lesions of the structure of the bones,—and, I say, manifested after a time, because in some cases the general disorder unequivocally precedes the local changes, and before these latter occur, we can predicate that they will occur.

In some books, rickets is classed among diseases of the bones. This is a mistake; rickets is no more a disease of the bones, than is typhoid fever a disease of the intestines. Rickets leads to disease of the bones in the same way that typhoid fever leads to disease of Peyer's patches; but there is a general disease preceding and accompanying the disease of the bones in the one case, as there is preceding and accompanying the disease of the intestines in the other. The change in the bones is the anatomical character of rickets.

The most constant and striking anatomical lesions in rickets are,—

1. Enlargement of the ends of the long bones,—of the parts where the bone and cartilage are in contact, *i. e.*, where the cartilage is preparing for ossification, and where ossification is advancing in the cartilage.

2. Softening of all the bones.

3. Thickening of the flat bones, *e.g.*, the bones of the skull—the scapula.

4. Deformities which follow from mechanical causes acting on the softened bones, *e.g.*, the deformities of the thorax, pelvis, spine, long bones.

5. Arrest of growth, not only of the bones, but of all the parts directly related anatomically and physiologically to the bones, *i. e.*, of the muscles, vessels, nerves, and teeth.

6. Certain lesions of the pericardium, lungs, and capsule of the spleen, the direct consequences of the thoracic deformity.

7. Less constant, but highly important changes, most commonly affecting the nutrition of the brain, spleen, liver, lymphatic glands, and muscles, and now and then of every organ.

[Dr. Jenner then passes on to examine at greater length the anatomical lesions of the bones, the consequences of this disease.]

1. *Enlargement of the Growing Ends of the Long Bones.*—[Dr. Jenner, under this head, by actual measurements in several cases, shows that the enlargement of the ends of the bones is actual, and not apparent only, as asserted by some.]

As to the intimate anatomical structure of the large ends of the bones : there is excessive formation of the structures which precede or form the nidus for ossification, there is retardation and incomplete performance of the process of ossification. In the healthy child the ends of the long bones measure more in circumference than the shafts, as the process of ossification is completed, the bone diminishes in girth.

In rickets there is an exaggeration of the condition we find in the first stages of ossification in the healthy subject, the completion of the process only is stayed. There is great development of the spongy tissue of the head of the bone, and of the epiphyses, and also of that layer of cartilage in which the primary deposit of calcareous matter takes place.

The layer of cartilage in which the cells are arranged in linear series instead of being half-a-line, is from a quarter to half-an-inch in breadth.

Again, the calcareous granular deposit is wanting at the boundary of ossification, and there the cartilage-cells calcify before the matrix ; the consequence of this is that you see the cartilage-cells, being apparently converted into lacunæ and imperfect canaliculi.

You may easily trace all stages in the deposit of calcareous matter on the inside of the cells, from that in which it forms a mere ring to that in which it is so thick as to leave only a vacant space resembling an almost perfect lacuna. Kölliker has generalised from his observations on ricketty bone, thinking that in rickets the normal process by which the lacunæ are formed is visible. My observations lead me to quite another conclusion—the calcification of the cartilage-cells in the growing cartilage in rickets seems to me identical with the calcification of the same parts occasionally seen in enchondromata. It is a pathological process, a petrefaction. The spongy tissue is much more spongy in appearance than natural, and from the interstices of its meshes a deep red pulp is expressible. This pulp is composed of colourless nucleated cells usually containing only one nucleus, now and then two, and occasionally several blood-globules, and in some cases a very large quantity of free fluid fat. If, as Sharpy, Somes, and De Morgan have supposed, these cells play an important part in the completion of the process of ossification, and I am—from my own observations on ricketty bones, &c.—inclined to think they do, we see in their abundance in ricketty bones only a further evidence that in ricketty bone there is excessive preparation for the process of ossification and arrest of the completion of the process. The periosteum is thickened over the head of the bone as over the bone generally. It attains its maximum degree of thickening just at the point of junction of the bone with the cartilage.

The periosteum of the whole bone is often more vascular and thicker than natural, but in the many post-mortem examinations of extreme rickets which I have made, I have never seen any bloody fluid as described by Guerin beneath that membrane. A crimson pulp fills the canal, and all the interstices of the tissues of the long bones. It is composed of elements identical in appearance with those expressible from the spongy tissue of the head of the bones. In this pulp, however, I have never observed any such quantity of free fluid fat, as I have noted in the heads of the bones near the line of progressing ossification.

2. The softening of the bones is sometimes so great that the bones which in their healthy condition are the strongest, may be bent by the most trifling force, and those which naturally are the thickest may be cut by a knife with facility.

This softening of the bones is chiefly the consequence of the diminution of their earthy salts. It seems, however, from the experiments of Lehmann and Marchand, that the animal matter of ricketty bones differs, in some cases, from that of healthy bone; for, in some of their experiments, the bones yielded no gelatin on boiling.

Putting together the results of the analyses of several observers, we gather that the bones of healthy children yield about 37 parts of organic and 63 of inorganic matters; and that those of ricketty children yield about 79 parts of organic and 21 of inorganic matters.

3. The thickening of the flat bones is the consequence of thickening and increased vascularity of the periosteum, and of the abundance of the nucleated cells, which, with blood form the pulp, which occupies all the meshes of the bone. The thickening is usually greatest just within or at the growing margin of the bone, so that in the cranial bones it is greatest near to or at the sutures, and least at the centres of ossification.

4. *Deformities which follow from Pressure on the Softened Bones.*—The spine is bent; the cervical anterior curve is increased; the face is directed upwards; and the head falls backwards. This curvature is only strongly marked when the muscular debility of rickets (of which I shall speak hereafter) is very decided. The vertebræ being softened and the muscles weakened, the head is no longer supported, and it falls forwards or backwards as circumstances may determine. Usually the child favours the falling of the head backwards, in order that it may see what is going on around, just as when suffering from paralysis of the third nerves, the child sits or walks with the head thrown backwards, in order that it may see under the fallen upper eye-lids. There is a posterior curvature of the spine, if the child is unable to walk, commencing at the first dorsal and extending to the last lumbar vertebra. If the child walks, then the posterior curvature is limited to the dorsal region, and there is an anterior curvature in the lumbar region. This posterior curvature in the child yet in arms, is sometimes so extreme that it may be easily mistaken (and I have known it mistaken) for angular curvature. They are distinguished thus:—

If the child be held by the upper part of its trunk, the weight of the lower limbs will usually remove the ricketty curve, and it may certainly be straightened if the nurse hold the child by the upper part of the trunk and the physician raises the lower limb with one hand and at the same time places the other on the curved spine.

This curvature of the dorsal and lumbar spine in rickets is the consequence of the muscular weakness, and softening of the bodies of the vertebra; its direction is determined by the weight of the head, &c. It is an exaggeration merely of the curvature always existing when the child of three or four months old is sitting unsupported on the nurse's arm. Lateral curvatures in the young child are less common than the antero-posterior. Their direction is determined by the position accidentally assumed by the child. For example, if the child be carried on the left arm constantly, there is a disposition to lateral curvature, and the convexity of the curve will be towards the left.

The femur is curved forwards and outwards. This curvature is produced before the child walks, by the weight of the legs and feet. The child sits on its mother's lap, or on a chair, and the lower extremities hang pendant. The bone being flexible, yields. After the child walks, the weight of the trunk is the chief agent in determining the curvature of the femur. The curve which existed before walking will be exaggerated.

When the tibia curves before the child walks or has been placed on its feet. the curvature is almost always outwards—an exaggeration only of the normal curve in the young child, and is produced by the child sitting somewhat cross-legged, and bearing on the floor or bed with the outer malleolus. After walking, the weight of the trunk is the chief agent in determining the bending of the tibia, and the direction of the curve will depend on the circumstances which determine the point on which the chief amount of pressure is brought to bear.

The curvature of the ulna and radius has been attributed to muscular action—to the child placing its arm around the breast of the mother, &c.; these causes are altogether inefficient for its production. The curvature of the bones of the forearm is produced by the child, owing to want of muscular power to support itself in the sitting posture, throwing part of its weight on to its arms. The child places its open hands on the chair, bed, or floor, and throws a large share of the weight of its trunk on to the bones of the upper extremities; the bones of the forearm are twisted as well as curved outwards.

The humerus is sometimes bent at an angle just where the deltoid is inserted. This curve is produced by the weight of the arm when the limb is raised by the action of the deltoid, and is increased by the cause which determines curvature of the forearm.

The clavicles are often the subject of extreme angular curvature. The chief bend is always at the same spot, viz., just outside the part to which the sterno-cleido-mastoideus, and the pectoral muscles are attached. The second bend is about half-an-inch from its scapular articulation. The first curve is forwards, and somewhat upwards; the second backwards. The curvature of the clavicle is produced partly by the weight of the arm on the humeral end of the clavicle—the sternal end being supported by the muscles just mentioned, and by its ligaments,—but chiefly by the force brought to bear on it, when the weight of the trunk is thrown on to the upper extremities, the child being in the sitting posture, with the hands on the floor or crawling.

The deformity of the greatest interest to the Physician is that of the thorax. The back is flattened. The ribs are bent at an acute angle where the dorsal and lateral regions unite. At that part the lateral diameter of the thorax is the greatest. From it, the ribs pass forwards and inwards to the point where they unite with their cartilages; on that line the lateral diameter of the thorax is the least, the cartilages curving outwards before turning in to unite themselves to the sternum. The sternum is thrown forwards, and the antero-posterior diameter of the thorax is abnormally great. The consequence of the direction of the ribs being inwards, and of the cartilages outwards, is, that the thorax is grooved from above downwards on its antero-lateral from the 1st to the 9th or 10th rib; the deepest part of the furrow being just outside the nodes formed where the ribs and cartilages unite. This groove extends lower on the left than on the right side, but it is deeper on the 5th and 6th ribs on the right than on the left side; the heart and the liver respectively supporting, to some extent, their corresponding ribs. The points of maximum recession correspond to the 5th, 6th, and 7th ribs. A little below the level of the nipple the chest expands considerably, the chest-walls being borne outwards by the liver, stomach, and spleen. If we examine the thoracic walls from the inside, the appearance is most remarkable, where the ribs join with the cartilages there are much greater projections than on the outside; but the 11th and 12th ribs, which are not inflexed, have the same enlargement on the inside as on the outside.

The great determining cause of the thoracic deformity is atmospheric pressure; this is aided by the elasticity of the lungs. How is the pressure of the atmosphere brought to bear on the thoracic parietes? Suppose the external thoracic parietes were made of cast iron or any other unyielding material, then the diaphragm could descend only so fast as the air could enter at the orifice of the larynx, and overcome the elasticity of the lungs. The thoracic parietes, however, in their normal condition, are not absolutely unyielding; but then there is a due relation between their strength, the power of the diaphragm and the rapidity of its contractions, the size of the orifice of the larynx, and the elasticity of the lungs. The chest-walls being healthy, and the orifice of the larynx of normal size, if the young child sobs violently —*i. e.* contracts the diaphragm with abnormal rapidity and force—the most flexible parts of the thoracic parietes will fall in during inspiration.

If the orifice of the larynx be narrowed, and if the diaphragm contract with only normal rapidity and force, there will be recession of the softer parts of the chest-walls at each inspiration.

Again, if the orifice of the larynx remain normal, the diaphragm act as energetically as in a healthy child, and the chest-walls be softened, then, at each inspiration, there will be recession of the most yielding part of the thoracic walls.

It is this last condition which exists in the ricketty child. The part of the rib where ossification is imperfect and incomplete is so soft that, at each descent of the diaphragm it recedes, and the furrow of which I have spoken is the consequence. Just in proportion as the ends of the ribs are forced inwards the sternum is carried forward, and the consequence is the remarkable form of thorax of which a model is on the table, of which some examples are now in the Hospital, and of which it is rare to pass a day without seeing cases in the out-patient's room. Rokitansky has maintained that this deformity of the thorax is the consequence of the want of power in the inspiratory muscles. I have repeatedly dissected subjects in which this deformity was strongly marked, and find that there is no correspondence between the points of insertion of the muscles of inspiration attached to the outer surface of the chest-walls and the points of recession.

The preparations on the table bear out my assertion. Weakening of these muscles would have as its consequence general want of expansion of the lungs, and the thorax would have the form that it obtains in senile atrophy—viz. long, with narrow antero-posterior and lateral diameters.

Again, the diaphragm is said to cause the circular recession by its direct action—by drawing in the receding parts at each contraction. On the table are some dissections, which prove, when compared with the cases and the model, that the line of recession does not correspond to the points of attachment to the diaphragm. But it does correspond to the upper margin of the liver, spleen, and stomach; and is produced—as the longitudinal furrow is—by atmospheric pressure: the parts of the parietes below being prevented receding by the organs I have just mentioned. The influence of the organs beneath in preventing the recession of the chest-walls is illustrated by the *apparent* bulging of the precordial region in every case of well-marked ricketty thorax. The chest-walls covering the heart do not recede so much as on the opposite side, and the consequence is that the left side is much fuller than the right; and, at first sight it might be supposed that there was abnormal fulness of the precordial region.

In excluding muscular action from all direct share in the production of curvature of the long bones in rickets, I am, so far as I know, unsupported by any authority.—*Med. Times and Gazette, March* 17, 1860, *p.* 261.

123.–ON HEMORRHAGE FROM THE BOWEL IN CHILDREN AS A SIGN OF POLYPUS OF THE RECTUM.

By Thomas Bryant, Esq., F.R.C.S., Assistant-Surgeon to Guy's Hospital.

[Polypus of the rectum is usually considered a rare affection; but within the last two years at least twenty examples have fallen under the author's notice. The connexion between hemorrhage from the bowel in children, and polypus of the rectum, is remarkably constant. Mr. Bryant says:]

In the majority of the cases which I have observed, the disease has existed for many months; they have all occurred in children under ten years, and in most the disease has been regarded and treated for piles. In some cases the discharge of blood from the bowel is constant, and its buttocks smeared with a bloody mucus. In these instances the polypus will generally be found to be within, if not protruding from, the sphincter. In other examples, occasional discharges of blood will be observed, although not to any very great extent, and this discharge will generally accompany and follow the act of defecation. In others, again, the hemorrhage will take place independently of any such process. There will generally be some straining after stool, but I have never observed any prolapse of the rectum; and although this disease is troublesome to the child, and of course debilitating, by the repeated, if not constant hemorrhage, when once recognised it is easily treated, and rapidly cured.

The recollection that such a disease is not so uncommon, and that it is always associated with hemorrhage from the bowel, should at once lead the surgeon to examine the part with his finger, when the growth will probably be easily detected. It is generally situated about one or two inches up the bowel, and will be found to vary from the size of a pea to that of a large nut. In some cases more than one will be present; they are always very moveable, and easily slip away from the finger upon anything like pressure; and, at times, some little difficulty is experienced in fixing them for removal. Such a practice is the only correct treatment, being invariably followed by a successful result. It may be done by means of forceps or ligature; and in many cases I have broken the polypus off its attachment by hooking my finger round its pedicle. No bad result has ever followed. The pedicle is always very slender, although it may be an inch or more in length.

The structure of the polypus is very simple, microscopically presenting the ordinary characters of the fibro-cellular growths. After removal, no subsequent treatment is required, and recovery may confidently be expected; the rectum, however, should be carefully examined, so that a second polypus be not overlooked.

I have thus briefly brought this small but not unimportant subject before the notice of the profession, feeling confident that the existence of polypus of the rectum is not so uncommon as is generally believed, and that such an affection is usually mistaken and treated for piles. In children, the presence of bleeding from the rectum should at once lead the practitioner to suspect the existence of a polypus; and, when detected, its removal is the only correct treatment. I have never had an opportunity of seeing a child suffering from piles, and believe that cases so described are generally mistaken, and that, in reality, they are cases of the disease now under consideration.—*Lancet, Nov.* 26, 1859, *p.* 530.

MISCELLANEOUS SUBJECTS.

134.—A TEMPERATE EXAMINATION OF HOMŒOPATHY.

No. III.—On the Statistics of Homœopathy.*

By W. Braithwaite, Esq., Leeds.

In this pamphlet we shall endeavour to show how the practitioners of homœopathy have endeavoured to mislead the public with respect to the success of the regular practitioners of medicine. And, by-the-bye, we wish to make the distinction clear between the practitioner of homœopathy and the practitioner of medicine. We do not consider that the homœopathic practitioner is a practitioner of medicine at all, in the strict sense of the word. He only practises a particular creed, differing altogether from the general belief of the medical profession. We are borne out in this interpretation by Mr. W. T. Harding, the Registrar of New Brunswick, under the New Medical Act. It appears that the homœopaths are delighted to have a college of their own. It is proper that they should, and we hope they will immediately desist using and priding themselves on degrees obtained in the regular colleges of medicine in this country, and obtained by answering questions in a way which many at the time disbelieved, and which others have since repudiated. It is a pity they still wear honours thus obtained. The college of Cleveland, New Brunswick, is the college to which we hope the homœopath will in future resort. But Mr. Harding says that this is not strictly a medical college, but only a homœopathic college; and that the degree is not a medical degree, but only a homœopathic degree. He says that, although a man from this college may be, and is called a Doctor of Medicine, "the degree is a homœopathic degree, and not a medical one. As well might a veterinary surgeon claim to be registered because his diploma contains the word 'surgeon'." Mr. Harding adds, "I think from what I can gather, that should the matter be brought to trial, the law will sustain the view that a homœopathic degree is not a medical degree in the sense of the terms of our act." (*Lancet, Oct.* 8, 1859, *p.* 368.)

Before placing before the reader the various statistical tables which we have collected from various writers, we will give one published in Leeds by a homœopathic practitioner, and circulated widely, both privately and by post, to show the extraordinary success of his own mode of practice, when compared with that of his neighbours. When we first saw this table at the end of his pamphlet, we thought it resembled some of the advertisements in the newspapers, such as Dr Barry's method of curing 50,000 cases without any medicine at all, and we could not but feel sorry that any individual should take such a step, when it did not at all refer to his arguments. This table is as follows (*Dr. Craig's Pamphlet, p.* 24):—

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* This Article is published as a separate pamphlet, price 3d., and may be had of Simpkin & Co., London, and of Mr. D. I. Roebuck, printer, Leeds. Also No. 1, price 2d. —No. 2, price 4d.—or the Set of three for 8d.

"PER CENT OF DEATHS IN ACUTE DISEASES.

Disease.	*Homœopathic.*	*Allopathic.*
Inflammation of the Lungs - - -	5·7	24
Pleurisy - - - - - - - - - - -	3.	13
Inflammation of the Bowels - - -	4.	13
Dysentery - - - - - - - - - -	3.	22
Typhus (Abdominal) - - - - -	15.	19
All diseases - - - - - - - - -	5.8	12.4"

We may look upon this table as a fair specimen of the way in which the public has been misled for some years; and we will now endeavour to throw some light on its fallacy.*

We consider this and other tables published by homœopaths, with the view of throwing discredit on the regular practice of medicine by surgeons and physicians, are highly discreditable, and ought to be disowned by the more honourable of this sect. How can their tables be true, even without any more evidence of their fallacy, than the following, respecting small-pox? Perhaps few diseases have ever been so fatal as this disease before vaccination was discovered. We may consider that almost every case was a dangerous one; and yet out of every hundred cases within the past century, 80 would, on an average recover. The following table will show this:—

Observer.	Place of Observation.	Per centage of Deaths.	Years.
Haygarth - -	London - -	16 - - -	1759—1768
Watt - - -	Glasgow - -	16.82 - - -	1783—1800
Dobson - - -	Liverpool - -	18 - - -	1772—1774
Percival - -	Manchester -	15 - - -	1769—1774
Nettleton - -	Eleven Towns in Yorkshire -	19 - - -	
Jurin - - -		18 - - -	
Heberden - -	London - -	8½–9 - - -	

(*Edin. Monthly Journal, Feb.*, 1860, p. 713).

We thus find that, in a disease of the most fatal description, less than 20 per cent. died in large towns; and at one period, calculated by Dr. Heberden, less than 10 per cent. So that if every one were attacked by small-pox instead of the common diseases of mankind, more would recover than what the homœopaths state as the recoveries by means of regular practitioners! This is so ridiculous, that it throws discredit on all their other statements.

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* In the compiling of this pamphlet we are indebted chiefly to Dr. Routh's work on the "Fallacies of Homœopathy"; Dr. Simpson's work on "Homœopathy"; Mr. Thomson on the "Statistics of Hospitals"; Dr. W. T. Gairdner's papers on "Homœopathy"; and Dr. John Taylor's "Cholera Report", and various journals, all of which we have endeavoured to acknowledge in due form and place.

After reading a good deal about the statistics of diseases, treated both by regular physicians and surgeons, and by homœopathic practitioners, we come to the conclusion that simple *numbers* of cures of cases are no criterion whatever of success in practice. We could prove that one physician who lost twenty-five cases out of every hundred which applied to him, was much more successful than another who lost only six. This can be easily shown to be possible. For example, one physician, celebrated in his town and neighbourhood, has passed a long career of professional success—a hundred patients apply to him, all *very severe cases, most of them likely to die,* and yet he is the means of curing 75 out of these severe cases. His death-rate in fact is 25 per cent. His apparent large death-rate is in fact owing to his severe cases, and to his celebrity and success. Another physician, known only in his own street or little neighbourhood, has likewise his hundred cases which apply to him in the course of a year. These are all mild cases. They may have the same *names* as the severe cases of the other physician, such as pneumonia, bronchitis, &c., but there was no likelihood, if proper treatment had been adopted, that *any* would die, and yet six out of the hundred die. The first physician, therefore, saves 75 bad cases out of his 100. The second physician loses 6 cases, when, in fact, he ought to have lost none: he not only did not save a single bad case, but he lost 6 cases, which, in all probability, the first physician would have saved—so that the first physician, losing 25 per cent., was much more successful than the second, who lost only 6 per cent. Mere *numbers*, therefore, can never prove success in practice.

This remark is applicable both to individual physicians and to hospitals. Success is proved by curing bad cases, and not those which would generally cure themselves in time. In calculating the death-rate in large metropolitan hospitals like Guy's, St. Thomas's, and Bartholomew's, in London, and the vast hospitals of Vienna and Paris, it ought to be remembered that they are often like immense poor-houses—the receptacles of aged and infirm persons, and most incurable cases. The splendid revenues of the London hospitals are well spent in thus relieving the worst cases which can afflict mankind. The hospitals of the Continent are generally paid by the state in which they are situated. The *beds* provided for the sick and aged alone, in Paris, for this year (1860) are as follow:—

For the sick	7,172
For the infirm and old	7,838
	15,010

When we think that in Paris alone above fifteen thousand beds are prepared this year for the sick and aged, we may easily imagine that in such a population a large death-rate may take place without any reflection on the skill and humanity of about 235 physicians and surgeons who are employed. (*Med. Times and Gaz., March* 10, 1860, *p.* 238.) Now we think that we shall be able to prove that the cases admitted into the hospitals of large cities as London, Paris, Vienna, &c., are much more severe than those admitted into homœopathic hospitals, and yet, taking the cases altogether, that the death-rate is remarkably low. Our object, however, in these pages, is not so much to prove the success of regular practitioners of medicine as the fallacy of homœopathic statistics. When we find homœopaths publishing their statistical tables, and circulating them through the country in a boastful spirit, depending simply on numbers, and not on the severity of the cases, we might feel disposed to turn to the columns of the newspaper where Morison and such like men advertise their cures, to see if these said homœopathic tables are not advertised. We find them not there, but in small pamphlets, or at the end of a pamphlet, placed in such a position that they appear to great advantage when compared with the tables published by large hospitals. Such a proceeding, however, is neither honourable nor dignified. It appears too much like quackery to meet with the approval of right-judging men.

We propose to show from tables to which the reader can refer for himself:—

1st. That hospitals of large cities, as London, Paris, Vienna, &c., have a large mortality, some more, some less, and variable from year to year, *owing to the great severity of the diseases admitted.*

2nd. That homœopathic hospitals admit a large proportion of mild cases, such as would never be admitted into regular hospitals, and yet have a large death-rate, amounting almost to the death-rate of the large hospitals of London, and a much larger death-rate than such cases ought to show.

3rd. That the provincial hospitals of England, including all kinds of cases, accidents of all kinds, and surgical operations, have a much smaller death-rate than can be shown by homœopathic hospitals.

4th. That the army and navy reports (the most correct that can be given) show a *much less* death-rate than homœopathic hospitals.

5th. That homœopathic statistics, even with respect to numbers, are not to be trusted, as some are known to have been *cooked,* and are here referred to in proof of this accusation.

In some of the calculations the numbers vary in the same hospital, and may thus seem to be contradictory, but this will be owing to the death-rate belonging to different years.

First, then, we admit that in the larger hospitals of London, Paris, Vienna, and other large cities, the death-rate is larger than in homœopathic hospitals, yet this is no proof of the success of homœopathic treatment, but only shows the severity of the diseases admitted.

There is no single individual in this country more esteemed and beloved than Miss Nightingale. What does she say on this subject in her book just published ? *

"No one, I think, who brings ordinary powers of observation to bear on the sick and maimed can fail to observe a remarkable difference in the aspect of cases, in their duration and in their termination in different hospitals. To the superficial observer there are two things only apparent—the disease and the remedial treatment, medical or surgical. It requires a considerable amount of experience, in hospitals of various constructions and varied administrations, to go beyond this, and to be able to perceive that conditions arising out of these have a very powerful effect indeed upon the ultimate issue of cases which pass through the wards.

"It is sometimes asserted that there is no such striking difference in the mortality of different hospitals as one would be led to infer from their great apparent difference in sanitary condition. There is, undoubtedly, some difficulty in arriving at correct statistical comparison to exhibit this. For, in the first place, different hospitals receive very different proportions of the same class of diseases. The ages in one hospital may differ considerably from the ages in another. And the state of the cases on admission may differ very much in each hospital. These elements, no doubt, affect considerably the results of treatment, altogether apart from the sanitary state of hospitals.

"In the next place, accurate hospital statistics are much more rare than is generally imagined, and at the best they only give the mortality which has taken place *in* the hospitals, and take no cognisance of those cases which are discharged in a hopeless condition, to a much greater extent from some hospitals than from others.

"We have known incurable cases discharged from one hospital, to which the deaths ought to have been accounted, and received into another hospital, to die there in a day or two after admission.

"Again, the sanitary state of any hospital ought not to be inferred solely from the greater or less mortality. If the function of a hospital were to kill the sick, statistical comparisons of this nature would be admissible. As, however, its proper function is to restore the sick to health as speedily as possible, the elements which really give information as to whether this is done or not, are those which show the proportion of sick restored to health, and the average time which has been required for this object; a hospital which restored all its sick to health after an average of six months' treatment, could not be considered as by any means so healthy as a hospital which returned all its sick recovered in as many weeks. The proportion of recoveries, the proportion of deaths, and the average time in hospital, must all be taken into account in discussions of this nature, as well as the character of the cases and the proportion of different ages among the sick.

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* "Notes on Hospitals," &c.

"Hospital mortality statistics give little information on the point, because there are elements in existence of which such statistics take no cognisance. In one set of metropolitan hospitals, for example, I find the mortality about two and a half per cent. upon the cases treated, while in other metropolitan hospitals the deaths reach from about twelve to sixteen per cent. To judge by the mortality in these cases would be most fallacious. Because in the first class of hospitals every ailment, however slight, constitutes a title to hospital admission, while, in the latter class of hospitals, special diseases only, at all times accompanied by a high rate of mortality, are admitted. Hence the duration of the cases admitted, and the general course and aspect of disease, afford important criteria whereby to judge of the healthiness or unhealthiness of any hospital in addition to that afforded by the mortality statistics." (*Edinburgh Med. Journal*, *Nov.* 1859, *p.* 451.)

In another part of her book Miss Nightingale says:—

"I have heard a doctor condemned whose patient did not, alas! recover, because another doctor's patient, of *different sex*, of a *different* age, recovered from a *different* disease in a *different* place. Yes; this is really true. If people who make these comparisons did but know (only they do not care to know) the care and preciseness with which such comparisons require to be made (and are made) in order to be of any value whatever, they would spare their tongues. In comparing the deaths of one hospital with those of another, any statistics are justly considered absolutely valueless which do not give the ages, the sexes, and the diseases of all the cases. It does not seem necessary to mention this. It does not seem necessary to say that there can be no comparison between old men with dropsies and young women with consumptions. Yet the cleverest men and the cleverest women are often heard making such comparisons, ignoring entirely sex, age, disease, place; in fact, all the conditions essential to the question. It is the merest *gossip*."

In another place this eminent lady gives her opinion of homœopathy in the following words:—

"Homœopathy has introduced one essential amelioration in the practice of physic by amateur females; for its rules are excellent, its physicking comparatively harmless—the 'globule' is the one grain of folly which appears to be necessary to make any good thing acceptable. Let then women, if they will give medicine, give homœopathic medicine. It won't do any harm." (*Edinburgh Med. Journal*, *March* 1860, *p.* 847.)

These remarks of Miss Nightingale fully bear out what we have been stating, and show the utter fallacy of mere numerical statistics as proving success in treatment.

The opinion of Dr. W. Stone, medical registrar of St. Thomas's Hospital, is much to the same effect. Dr. Stone says:—"Thus it comes about, that wherever a Hospital is in the greatest demand, its occupants will be the most dangerously ill, and its death-rate the highest proportionally to its admissions. A hospital death-rate is thus not a measure of the cures performed, or of its internal healthiness, so much as of the number and intensity of cases of serious disease in its district, and of the general anxiety among the lower classes to avail themselves of its benefits. It is physically what the House of Correction is morally—the '*sentina reipublicæ*,'—and in the present state of society, the more foul its contents, the more efficiently is it doing its duty.

"Indeed, largeness of death-rate, so far from being a sign of defective administration, or of bad sanitary condition in a hospital, on the medical side at least, is of quite an opposite import. This is exemplified in St. Thomas' by what are called the Clinical Wards, to which are brought at all hours, cases marked by the greatest urgency, requiring the closest attention, and affording the best school of instruction for the pupils. The death-rate in these wards is high, although it is obvious that in them more critical points of practice occur, and more real cures are brought about than can be attained in wards full of chronic disease, where the death-rate is much lower. But a far stronger evidence is found in the examination of death-rates in specific diseases. For this purpose a few have been selected from the Report for the year 1858, which I have recently had the honour of presenting to the hospital authorities, and in which every case has been verified by personal observation of the reporter, independent and corroborative of the diagnosis appended to the bed-ticket.

"From this it appears that the death-rate of typhus fever was 18·1 per cent.; in typhoid, 20·0 per cent.; in delirium tremens, 17·3 per cent.; in pneumonia, 17·0 per cent.; in diarrhœa, 16·0 per cent.; in erysipelas, 7·0 per cent.; and in poisoning cases, accidental or suicidal, 17·6 per cent. All these death-rates are very high, indeed; yet no person of any experience can deny that these are, of all others, types of disease which benefit by the accurate diagnosis, bold medication, liberal dietary, abundant stimulants, and careful nursing of a hospital. Probably the chief medical triumphs of the year will be found among the cited instances; undoubtedly more lives have really been saved among these patients than among any other equal number. And yet the death-rates are some of the very highest of any on the register.

"This seems to complete the appeal from blind figures to discriminating intellect. Abstract death-rate is evidently no fair method of testing the sanitary condition of a hospital, whatever be its value in estimating the healthiness of a district. And it is always to be regretted that fallacious reasoning should be put forward in defence of an object which actually stands on more solid grounds and offers some real advantages." (*Med. Times and Gazette, Nov.* 12, 1859, *p.* 490.)

The death-rate in these large hospitals, therefore, ought to be judged according to the *severity*, and not the *numbers*, of the cases. If you wish to judge by numbers only you ought to include the milder *out*-patients, and this would reduce the death-rate to a remarkably low figure. Thus in the Leeds Infirmary, the death-rate on all cases admitted since 1767, is only about 3·2 per cent, and in the Leeds Dispensary, 4·02.

Take also the statistics of Guy's Hospital for any one year, that for 1854, for example, which is now before us. We find the following numbers referring to in-patients.

Rate of mortality over all the cases (during the year), 8·8 per cent., including both surgical and medical.

Rate of mortality in medical cases, 5·9.—(But this is wonderfully reduced when we add the 8350 milder out-patients.)

The number admitted in 1854, were	In-Patients . . .	5089
	Out-Patients . . .	8350
	Total	13,439

Of these nearly all the in-patients would be severe, at least the severe cases are picked out for admission, and the milder are admitted as out-patients. Taking both together we make the death-rate to be only 3·9 per cent.—not even calculating 21,710 cases of a more casual and slight description, which were attended to. Now when we see that out of 13,439 cases admitted into Guy's Hospital, or attended to as out-patients, and including the worst cases that can be found anywhere, only about 3·9 per cent. could be registered as having died, we are struck with the success of modern medical and surgical practice! It will be seen how nearly the success of Guy's hospital and the Leeds Infirmary correspond, when we take all the cases together. But as it can be shown that the cases admitted into regular hospitals are the *worst cases* that can be picked out from the masses which apply, so we think that it can be shown that the cases admitted into homœopathic hospitals are the *mildest* that can be selected, and yet that the death-rate is very high for such cases. It is shown in a paper by Mr. J. Thomson, on the Statistics of Civil Hospitals, that the average mortality of 30 provincial hospitals in England is 4·46 per cent, while 14 hospitals of London average 7·79 per cent.; and some, such as Addenbrook's hospital, Cambridge, has a mortality of only 2·4 per cent., and Exeter 3·2. A great deal therefore depends on the *kind* of cases and not on the number admitted. The table on the opposite page, by Mr. Thomson, will explain this point.

This table embraces a long period, and confirms the opinion that statistics of hospitals must ever vary from year to year, according to the nature of the cases. It shows, however, that considering the reception of all kinds of cases, and exclusive of all out-patients, the mortality is very low. If the milder out-patients had been included, we believe the death-rate would be extremely low, TABLE showing the number of patients treated, the deaths, and ratio of mortality in the following fourteen Hospitals, at different periods, from data compiled from the British Medical Almanac and Dr. Hawkins' Statistics. (*Edin. Med. and Surg. Journal*, 1843, *p.* 355.)

Hospitals.	First.						Second.					
	Periods.	Years.	Treated	Died	Mortal. pr.cent.	Av. yly. Admis.	Periods.	Years.	Treated	Died.	Mortal. pr cent.	Av. yly. Admis.
Chester General Infirmary .	1826 to 1835	10	5331	220	4·1	533	1839 to 1841	2	1214	88	7·2	607
Royal Infirmary, Manchester,	1752 1836	84	74360	3876	5·2	885	1836 1837	1	1890	208	11·0	1890
Kent & Canterbury Infirmary,	1793 1826	34	15697	655	4·1	462	1827 1836	10	4239	198	4·6	423
St. Bartholomew's Hos. London	1790 1819	30	10186	903	8·8	339	1830 1834	5	4748	378	7·9	949
St. George's Hospital, . .	1830 1834	5	9500	1075	11·3	1900	1835	1	2192	226	10·3	2192
London Hospital, ...	1828 1833	6	13870	1622	11·6	2311	1835 to 1836	1	2517	260	10·3	2517
Westminster Hospital . .	1833 1835	3	2535	260	10·2	845	1835 1836	1	992	100	10·0	992
St. Thomas's Hospital, . .	1764 1813	50	—	—	6·6	—	1835 1836	1	3000	261	8 7	3000
Exeter Hospital, ...	1826 1835	10	10145	326	3·2	1014	1835 1836	1	1069	35	3·2	1069
Bristol Infirmary, ...	1826 1835	10	15805	1325	8·3	1580	1828	1	1689	160	9·4	1689
Gloucester Infirmary, . .	1829 1835	7	4253	241	5·6	607	1835 to 1836	1	646	33	5·1	646
Bath United Hospital, . .	1827	1	551	30	5·4	551	1835	1	814	47	5·7	814
Lincoln Hospital, . . .	1826 to 1835	10	2762	133	4 8	276	1836 to 1837	1	*320	14	·4	320
Addenbrook's Hos., Cambridge	1826 1835	10	6414	157	2·4	641	1832 1833	1	620	17	·7	620
Total . . .			171409	10823	6·31				25950	2025	7·79	

* Out and in patients.

The following table of *Continental Hospitals*, compiled from Dr. Hawkins' 'Elements of Medical Statistics,' *(Edin. Med. Journal*, 1843, *p.* 356), shows the number of patients treated, the deaths, and ratio of mortality per cent. in the following twenty-six hospitals:—

Hospitals.	Periods.	Years.	Treated.	Died.	Prop. deaths to the number treated.	Ratio Mortality per cent.
					1 in	
Hotel Dieu, Paris,...............	1770 to 1780	10	....	..	4	25.00
Do.	1822	1	....	..	7	14.25
The Hospitals of Paris, ...	1822	1	47393	..	8	12.50
Hospitals at Lyons.	1822	1	....	..	11	9.09
Hospital at Montpelier, ...	1822	1	....	..	10	10.00
The General Hospital, the Charite at Berlin,.....................	1796 to 1817	20	....	..	9	16.66
The Great Hospital at Vienna, (2000 beds),..................	1824	1	.....	..	6	16.66
Civil Hospital, Pesth, ...	1826	1	....	..	6	16.66
City Hospital at Dresden, ...	1816	1	....	..	7	14.25
Hospital at Munich, ...	1819	1	3500	..	9	11.11
Hospital at Heidelberg, ...	1825	1	285	...	21	4.76
Imperial Hospital at Petersburgh,	1803 to 1817	14	....	..	4.5	22.22
Do.	1816	1	2643	461	4.4	22.56
The Cantonal Hosp. Pays de Vaud	1825	1	344	16	21	4 68
St Pierre at Brussels, ...	1823	1	....	..	9	11.11
St Pieter's Gasthuis at Amsterdam	1798 to 1817	20	....	..	8	12.50
General Hospital at Genoa, ...	1821	1	1 dth. in	6 reco	veries.	..
St Giovanni at Turin, ...	1821	1	1 dth. in	7 reco	veries.	..
The Great Hospital at Milan,....	1823	1	13278	1901	7	14.31
San Matteo della Pieta, at Pavia,	1823	1	5287	..	10.5	9.52
Hospital at Padua, ...	1820 to 1821	1	149	10	15	6.66
——— at Bologna,............	1816 1819	3	453	35	13	7.73
——— at Leghorn, ...	1818 1825	7	....	..	7⅓	13.69
——— at Palermo, ...	1823	1	4221	515	8	12.25
The two principal Hospitals at Madrid, ...	1814	1	15230	1187	13	7.80
Do.	1818	1	14500	1283	11	8.85
Hospital at Valencia, ...	1786	1	4800	639	7½	13.31
The Hospital Santa Cruz, Barcelona, ...	1821 and 1822	2	8205	1293	6⅓	15.75

To show how variable may be the death-rate in hospitals, compare the above with Dr. Routh's table. (*Fallacies of Homœopathy*, *p.* 85.)

Year.	Place.	Admissions.	Deaths.	Mortality per cent.
1828-31 34-35	Limberg :—All Diseases. General Hospital	...	...	2 to 3
1838-39...	Göttingen Poliklinik	...	...	2 to 3
1837-38...	Stuttgard Catherinen	...	...	3 to 4
	Military :—			
1850 ...	Grenadier Guards ...	1,320	29	2·02
	Coldstream Guards ...	878	17	1·09
	Scots Fusileer Guards	602	11	1·08
	Royal Ordnance ...	4,977	38	0·76
	Dreadnought Ship ...	2,121	78	3.68

Year.	Place.	Admissions.	Deaths.	Mortality per cent.
	France :—All Diseases.			
1837-46...	Hôtel Dieu ...	11,130	...	8·06
	Annexe Hôtel Dieu...	5,914	...	7·06
	Pitie	10,427	...	7·01
	Charité	8,010	...	6·08
	St. Antoine	3,469	...	8·00
	Necker	4,769	...	8·03
	Cochin	1,982	...	8·04
	Beaujon	4,310	...	9·01
	Bon-Secours	...	...	5·05
1834-46...	St. Louis	8,240	...	4·06
	Enfans Malades ...	3,604	...	12·00
	Accouchemens ...	7,502	...	3·01
	Midi	3,730	...	3·05
	Lourcine	2,083	...	2·09
	Clinique	2,030	...	3·01

At one time, therefore, the death-rate in hospitals may be very large and at another time small, owing entirely to the nature of the cases. Now it appears at first sight that the death-rate in these hospitals is large, and especially in continental hospitals. But we maintain,

Secondly.—That the cases admitted into homœopathic hospitals are much milder than those admitted into regular hospitals, and yet have a larger death-rate than we find in English provincial hospitals.

If the reader will take the trouble to refer to an admirable letter of Dr. Gairdner's, in the Medical Times, for 1852, page 340, he will find this subject very candidly discussed. Dr. Gairdner takes the number of patients admitted into Fleischmann's hospital, Vienna, during 8 years, from 1835 to 1843, and compares these cases with nearly the same numbers admitted into the Edinburgh infirmary in 2 years, viz., 1842-3. In the Vienna hospital were admitted 6,501 ; of these 407 died, a mortality of 6·26 per cent.: while in the Edinburgh hospital 6,369 cases were admitted, and 758 died, the mortality being thus 11·9 per cent. How is this, it will be asked, that the Edinburgh hospital lost nearly double the number of cases? We will proceed to show the reason, and a more satisfactory one cannot be given, that mere *numbers* are fallacious. Out of six of the most fatal diseases which can attack the human body, let us compare those which were admitted into these two hospitals.

	Edinburgh.	Vienna Homœopathic.
Consumption - - -	276	98
Heart Disease (organic)	159	15
Bright's disease of Kidney	82	0 (!)
Paralysis - - - -	103	5
Apoplexy - - - -	14	9
Disease of Liver (organic)	33	1
	667	128

As Dr. Gairdner says, "all commentary upon this is unnecessary"!

"In order, however," says Dr. G., to make the conclusions which legitimately spring from this investigation more completely irresistible, I have had the curiosity to make a few further selections from the returns. I give below the remaining instances in which the Edinburgh proportion of cases materially exceeds the homœopathic. As if by magic, they turn out to be, with but two exceptions, the most intractable enemies with which the practitioner has to deal. They are as follows:

	Edinburgh.	Vienna Homœopathic.
Neuralgia	14	0
Internal Aneurism	18	1
Diabetes Mellitus	17	0
Amaurosis	15	0
Caries and Necrosis	57	5
Malignant Tumours	55	0
Other Tumours	36	0
Bronchitis (acute)	118	15
Rheumatism (acute and chronic) ...	343	188

"Of these last items, I have more to say immediately; meantime, by way of contrast, and to show, if possible, still more conclusively the principle on which homœopathic cases are selected for treatment, I shall reverse the picture, and give a list of the diseases which predominate to a large extent in Fleischmann's hospital.

	Vienna Homœopathic.	Edinburgh.
Chlorosis and amenorrhœa	90	48
Cholera	24	2
Colic	45	10
Diarrhœa	114	28
Dysentery	44	16
Erysipelas and Erythema	212	82
Gout	140	0
Hæmoptysis	50	8
Headache	61	37
Herpes	20	1
Inflammation of brain	17	8
Endocarditis	29	(?)
Pneumonia	300	83
Pleuritis	224	32
Peritonitis	105	19
Sore throat	301	34
Influenza	52	0
Chicken Pox	110	2

"The predominance of influenza is evidently owing to the period embraced by the returns having included an epidemic visitation of this disease. Possibly the number of cases of cholera, diarrhœa, and dysentery, (or what may have passed under the latter name,) and of erysipelas, may have a similar explanation. But what is the rest of this list, which forms the staple of the homœopathic experiment? Is it not composed, without an exception, of the *curable*, often of the *easily and constantly curable* diseases of the economy? Nay, is it not plain to the most ordinary allowance of common sense, that cases have been admitted by dozens, probably by hundreds, for no other purpose than to contribute to the success of the experiment, and to swell the triumph of homœopathy? I cannot imagine to what purpose else we have 300 cases of sore-throat, and 20 of herpes, diseases which are rarely, except in the most special cases, admitted into any of our great hospitals in this country, on account of the pressure of the more severe and fatal diseases to which, as shown above, our doors are thrown open, while our experimentalists turn their backs on them, or least give them the cold shoulder! To be sure they are ugly subjects for curative experiments, these same phthisical cases, and organic diseases of heart, liver, and kidney; and, whatever one may think of the honesty, no one can doubt, the prudence of giving the preference to sore-throats and shingles, as well as to catarrh, dyspepsia, colic, headache, and a host of the minor ills which will be found to be numerically strong in the returns.

"And now I assert, without fear of contradiction, that the homœopathic returns are not only void of triumph to the system, but that they cover it with disgrace. With such a selection of cases as I have shown above, I maintain they ought to have reduced their mortality to a far lower point than they have done. It is of no use to quote alleged cures of pneumonia or pleurisy, and to demand comparisons with 'the best hospital physicians who use allopathic remedies.' I think we are justified in believing that the cases of individual disease, like the general returns, are a sham and a fraud; and that the contrast between pneumonia at a homœopathic hospital, and pneumonia in the Edinburgh Infirmary, would be, if we could get to the root of the matter, as great as between the general lists in the one and the other hospital. Every one who has gone about the wards of an hospital in search of crepitant râles and dulness on percussion, knows that there is nothing so easy to find or so often cured as the slighter degrees of what may be technically called pneumonia; and as to pleuritis, if we may trust the evidence of *post-mortem* examination, its simpler forms must be of immense frequency; so that if our scrupulous experimentalists chose to place everthing which we commonly term rheumatic stitch under that convenient and formidable-looking designation, it would not be easy to prove them wrong. They have, however, betrayed themselves in one point;—in giving the cipher of 300 to pneumonia, and only 15 to the far more frequent disease, bronchitis, they have committed what, according to Napoleon, is 'worse than a crime—a blunder;' showing that it requires a more adroit management than even that of our experimentalists, to manufacture statistics of plausible and serious aspect from the miniature types of disease by which they (very judiciously) think proper to test the efficacy of their system.

"I feel that it is useless to enter into further details as to this statistical fraud. It is, I hope, abundantly evident, that, even supposing the numbers to be correctly stated, and the docketting of the cases to have been free from objection, the character of them, as reported, is such as to imply selection; and, on the other hand, it is next to certain, that no dependence whatever can be placed on the statements of the reports, in regard to the nomenclature of diseases. We have, therefore, only to deal with the fact, that an hospital in which there is reason to think that the vast majority of the cases were of the most trivial description, has a mortality of 6·26 per cent.; and that the interested partizans of the system therein pursued, demand for this result the palm of an unquestionable superiority, or (in the words of one of their leaders), proclaim it 'far beyond the reach of any other known method of treatment.' To this it is enough to answer, that many hospitals in England have an average mortality much below that above mentioned; I have already instanced three of these, (Canterbury, 2·0 per cent.; Cambridge, 2·4; Exeter, 3·2.) I might add, that of eight district general hospitals in Scotland, noticed in the article by Mr. Thomson, already referred to, three have a smaller mortality than the homœopathic institution, viz., Dundee, 5·10 per cent.; Aberdeen, 4·66 per cent.; and Inverness, 4·36 per cent. All of these hospitals are, like that of Edinburgh, recipients of a considerable proportion of incurable cases, and I do not believe that any of them admit 5 per cent. of cases of cynanche tonsillaris. I am not so well acquainted with the class of cases admitted into English provincial hospitals; but in a list of thirty of these institutions in the paper above referred to (from materials in the *British Almanack* for 1836-7,) *there are only two whose mortality is not less* than that of the Homœopathic Hospital of Vienna, and the average mortality of the whole thirty (4·46) is less by nearly a third. So that the unprecedented success of homœopathic treatment is not only a very ordinary and moderate success as compared with hospitals in general, but as compared with hospitals of the size of Fleischmann's (fifty beds) it would be found to be a positive failure; and doubly, trebly a failure, when we take into consideration all the facts revealed in the preceding part of this letter.

"Before concluding, I cannot resist alluding to one other subject,—I mean the proportion of cures. In the record of a death, it is impossible to show any bias, or in any way to deviate from accuracy without gross falsehood, with correspondingly great risk of detection. But, in the column of cures in this hospital may be read the character of the whole of its records. The alleged cures in the Vienna Homœopathic Hospital are 92 per cent. of the whole cases; and, as the deaths are

6·25 per cent., it follows that there is actually *scarcely any medium between death and cure!* To any one who knows what hospital cases are, or should be, this simple statement proves rather more than was intended. Compare it with the returns of any hospital which has no system to support—I choose Dumfries, simply because its mortality is identical with that of Fleischmann's Hospital :—

	Cures per Cent.	Deaths per Cent.
Fleischmann's Hospital	92·	6·26
Dumfries „	76·02	6·26

"Alas for the

'Vaulting ambition that o'erleaps itself,
And falls'

on the other side of truth and probability! In straining every nerve after this ideal and fictitious ratio of cures, Dr. Fleischmann unluckily forgot the following ugly dilemma: If, from the excellence of his art, or any other cause, he was enabled to cure 16 per cent. more than Dumfries, why was his skill not equally effective in reducing the mortality? There can be only two answers to this question, and we may give the homœopathists their choice of them. Either the cases were really curable in enormous proportion, and the homœopathic art is responsible for a mortality which may be considered, under these circumstances, quite appalling; or the alleged cures are a mockery and a delusion, inconsistent with nature and fact, and cunningly dressed up for the undiscriminating wonder of the multitude. To apply an uncharitable judgment of Dr. Fleischmann's to his own case, '*Curantur in libris—moriuntur in lectis.*'

"And now I leave the question of the results of homœopathic hospital treatment, without hesitation, to the judgment of 'common sense.' I only stipulate that 'common sense' will take the trouble to make herself acquainted with the facts of the case as stated and analysed above, and will protect and arm herself against sophistry and disingenuousness by an alliance with another equally useful personage, 'common honesty'." (*Med. Times and Gazette, vol.* 25, 1852, *p.* 342.)

These statistics of Dr. Gairdner were called in question by some homœopathic writers, owing to some slight mistake. But we cannot find that Dr. Gairdner's argument is at all weakened, but rather strengthened. In another letter, entitled, "Further Remarks on Homœopathic Hospitals, being a Second Sequel to the Edinburgh Essay on Homœopathy," Dr. Gairdner very candidly corrects his clerical mistake; but, in doing so, he seems to us to have added increased force to his statistics. He says :—

"The number of cases of catarrhal disease in Fleischmann's Hospital is by no means easily discovered from the returns. For, in the first place, 'Acute Bronchitis' is separately stated from 'Catarrh;' then, 'Influenza' is distinguished from Bronchitis; and 'Catarrhal Fever' from both. Besides these, there are 'Coughs' of every variety, without the least indication whether they are catarrhal or not; and a number of other even less definite items bearing on disease of the lungs; the whole of which, to prevent cavil, I transcribe below:

Catarrh	44 cases	Croup	1 case
Acute Bronchitis	15 ,,	Emphysema of Lungs	2 cases
Influenza	52 ,,	Hæmoptysis	50 ,,
Catarrhal Fever	175 ,,	Hydrothorax	7 ,,
Cough (chronic)	130 ,,	Œdema of Lungs	14 ,,
Cough (spasmodic)	18 ,,	Spasms of the chest	3 ,,
Cough (not otherwise defined)	9 ,,	Ulcer of Lungs	44 ,,
Asthma	2 ,,		

"The above cases include *every instance* of pulmonary disease, or anything resembling pulmonary disease, excepting those hitherto accounted for, that can be fished up out of Fleischmann's grand total of 6551 cases, as I find them displayed at length in a table in Drs. Drysdale and Russell's book on Homœopathy. The homœopathic reviewer insists upon it that I shall receive the whole of the cases under the first seven headings above as being equivalent to 'Catarrh;' he accordingly adds them up, and finds, with perfect accuracy, a total of 443. I might very well take him at his word, and show that he does not thereby get rid of the anomaly which discredits the homœopathic statistics. For while catarrh in the General Hospital of Vienna *outnumbers pleurisy and pneumonia, taken together, more than twofold;* in the Homœopathic Institution there are only, by the calculation even of the reviewer, extreme as it is, 443 catarrhs to 524 cases of pleurisy and pneumonia. A more signal proof of the correctness of my position, when tried by the severest test which can be applied to it, I could not wish to have. But since the whole returns are now in my hands, I shall not let off my opponents so easily; but shall place them in a far more conclusively ugly situation than they have yet occupied, in their defence of these most indefensible and untruthful documents. To this end, I shall leave all the details with which we have hitherto been engaged, to shift for themselves. What 'Catarrhal Fever' may be, as distinct both from 'Influenza' and 'Catarrh,' I shall not undertake to decide; neither shall I venture to guess how many or how few of the cases of 'Cough' may be legitimately brought under the general designation of Catarrh. Perhaps it would be still more difficult to separate the cases of 'Chronic Cough' from those of 'Phthisis,' to which, no doubt, the former heading ought to contribute a considerable proportion. It is at least evident that the real number of catarrhs in Fleischmann's returns is not to be fixed without cavil or objection on one side or other. I shall not, therefore, attempt to fix it at all, but adopt a different way of testing the question at issue.

"The reader will, I trust, recollect that the question raised by me was simply this—Do the returns justify the belief that the homœopathic cases of pneumonia and pleurisy were genuine cases; *i.e.*, like in kind and in degree to cases of pneumonia and pleurisy as observed in other hospitals? For, unless the cases be genuine and unquestionable, of course the alleged cures cannot be accepted as *real cures of the diseases alleged*,—viz., of *Pneumonia and Pleurisy*. And observe, it is the homœopathists, and not their opponents, who in fact have brought about this issue, by putting forward their cures of these two formidable diseases as a great and unanswerable proof of the superiority of their system of treatment.

"Now let the reader observe carefully the principle on which the following comparison is founded. I apprehend that it completely removes all possibility of cavil, and goes at once to the very root of the matter.

"I propose to show, that in the homœopathic statistics cases of pneumonia and pleurisy abound to a degree quite incredible, and quite inconsistent with the experience of others—even in Vienna itself;—that while in ordinary hospital practice, pneumonia and pleurisy form a small minority of the cases of pulmonary disease, in the homœopathic hospital they immensely outnumber their due proportion. This will appear quite clear from the following statements.

"In the Vienna General Hospital, the diseases of the lungs and air-passages are comprised generally under five heads—Catarrhs, Emphysema, Pleurisy, Pneumonia, Tuberculosis of lungs. Dividing these diseases into two classes, of which pleurisy and pneumonia form one, and all remaining diseases the other, we arrive at the following statistical result:—

Pleurisy and Pneumonia - - - -	1036 cases
Catarrhs, Emphysema, and Tubercle of Lungs	3462 ,,

In other words, the sum of pleurisy and pneumonia, in the Vienna General Hospital, amounts to *less than one-third* of the remaining diseases of the lungs and air-passages; these diseases comprising catarrhs and emphysema, with tubercular affections of the lung.

"To compare this result with that of Fleischmann's statistics, it will be necessary to add together the diseases separately stated above. They amount, as the reader may easily find for himself, to 566 cases of miscellaneous disease, under a great variety of titles; but including all cases resembling, however remotely, those stated in the Vienna General Hospital List under catarrh and emphysema; including also probably a proportion of the tubercular cases. If the sum of the latter be now completed by adding the 98 cases given under a separate heading, as 'Phthisis,' we shall have, beyond doubt, a list of all the pulmonary diseases in Fleischmann's returns, exclusive of pneumonia and pleurisy; and however little justice I may have done to my own side of the argument in this calculation, I defy the most dexterous manipulator of figures to show that the opposite side has not received the most ample consideration. What is the result?

"In the Homœopathic Hospital of Vienna, according to the preceding mode of calculation, we have

Pleurisy and Pneumonia, - - - -	524 cases
Catarrhs, Emphysema, Asthma, Phthisis, Hæmoptysis, Ulcer of lungs, Œdema of Lungs, Coughs, &c., &c., - - - -	644 ,,

So that while in the Vienna General Hospital the proportion of pleurisy and pneumonia to the remaining diseases of the lungs and air-passages is *less than one-third;* in the homœopathic hospital it amounts to *more than five-sixths !*

"To complete this view of the matter, I will shortly refer to the published statistics of the Edinburgh Royal Infirmary, during the years 1842–3, as drawn up by Dr. Peacock, and employed by me on a former occasion to test the homœopathic data. In these years I find that there were admitted—

Pleurisy and Pneumonia, - - - -	122 cases
Other diseases of the lungs and air-passages,	643 ,,

In other words, the cases of pleurisy and pneumonia conjointly amount to a considerably smaller proportion than in the Vienna General Hospital; or *rather less than one-fifth* of the remaining diseases.

"And to show that this is not the accident of an epidemic season, I may refer to the last Report of the Vienna General Hospital, which I received only a few days ago. By consulting a chart at the beginning of that report, I find that pneumonia has on the whole rather declined in frequency since 1852; but that it has maintained a tolerably equable level during the years 1853 to 1855; over the last of which the report extends in detail. During the year 1855 we have

Pneumonia and Pleurisy, - - -	1064 cases
Other diseases of the lungs and air-passages,	3498 ,,

In other words, the proportion of pneumonia and pleurisy to the other diseases mentioned amounts, as before, to *somewhat less than one-third.*" (*Dr. Gairdner on Homœopathic Hospitals*, *2nd Sequel.*)

Dr. Gairdner thus shows that in the Vienna Homœopathic Hospital a much larger number of cases were admitted which were *called* pleurisy and pneumonia than are usually admitted into other hospitals, and more than on any average of cases ought to have been admitted, unless cases were *called* so which were not in reality such as we should term pneumonia and pleuritis.

From these facts we think every candid individual will see that the homœopathic cases are of the mildest description, such as 300 cases of sore throat, 114 cases of diarrhœa, 212 cases of erysipelas and erythema, 140 cases of gout, 61 cases of headache, and 110 cases of chicken-pox, all of which are generally easily and rapidly cured.

What physician would ever think of admitting 110 cases of chicken-pox, or 300 cases of sore throat into an hospital? The reader will also see that the cases in the Edinburgh Hospital were of a much more severe character, attacking the lungs, heart, kidney, liver, and brain. Numbers, therefore, are comparatively useless in calculating success in treatment; and yet even in the death-rate of regular hospitals Mr. Thomson and Dr. Gairdner have shown that, notwithstanding this difference in the *severity* of the cases, many of them, especially provincial hospitals and the army and navy hospitals, show a much less percentage of deaths than can be found in homœopathic hospitals.

But what will be thought of the boasting statistics of homœopaths, if it can be shown that their cases, although so much milder, were *much longer in recovery* than in regular hospitals? It will be very sad indeed if we can show these three things—That they have more deaths (except in the large hospitals of London, Paris, Vienna, &c.); That their cases are much milder; and, That those which do recover are much longer about it than in regular hospitals! We have already shown that in the Leeds Infirmary, the Leeds Dispensary, and 30 provincial hospitals, the deaths are much fewer than in homœopathic hospitals; and that in the hospitals of large cities the high death-rate is quite consonant with great success in treatment, considering the danger of the cases; and even in those large hospitals, as in Guy's, if we include the milder out-patients the death-rate is reduced to little more than 3 or 4 out of every hundred patients who are attended to, not including the casual and unimportant cases. But we think that it can also be shown that homœopathic cases are much longer in recovering than those under regular medical treatment. The following table of Dr. Routh greatly strengthens this opinion, although we by no means look upon it as proved. It would be difficult to prove such

	Treatment before Convalescence	Residence in Hospital after	Total days Residence in Hospital.
Cases treated also Allopathically ...	10·1 days	7·0 days	17·1 days
Ditto exclusively Homœopathically	14·0	15·1 ,,	29·1 ,,
Ditto Allopathically, admitted in 1st stage	9·0 ,,	4·0 ,,	13·0 ,,
Ditto Allopathically, admitted in 2nd stage	10·5 ,,	6·5 ,,	17·0 ,,
Ditto exclusively Homœopathically, 1st stage	20·5 ,,	11·5 ,,	32·0 ,,
Ditto exclusively Homœopathically, scarcely 2nd stage ...	7·0 ,,	17·8 ,,	24·8 ,,
Ditto exclusively Homœopathically, 2nd stage	13·2 ,,	17·0 ,,	30·2 "

an opinion unless a regular physician and a homœopath had each cases *exactly alike:* but as we know that medical cases, like human faces, may bear a *general* resemblance yet never an *exact* one, so it would be impossible to *prove* that the homœopath was longer about his cures. This table at any rate casts a *suspicion* over the homœopathic practice. Dr. Routh says:—"The very imperfect action of the homœopathic treatment is apparent from this table, setting forth the duration of the pneumonia and residence in the hospital after convalescence. So far then, as an analysis founded on so few cases is to be depended upon, it is unfavourable to homœopathy. Even in the bad pneumonia cases in the Glasgow Infirmary the average residence was only 20 days, occasionally as high as 26 during the epidemic fever of 1847."

This opinion is confirmed by Dr. Lee whom we quoted at the end of our Pamphlet No. 2, p. 40, and whose words we will repeat here. He says:—"In looking over the history of several of the cases treated at the London Homœopathic Institution, I found, what might be anticipated, that they were very analogous to the above, viz., the ordinary slighter ailments usually met with in dispensary practice, which seldom require a long treatment, though most of the cases reported in the Homœopathic Annals required two, three, or four months' attendance before the patients were dismissed. Again, we have had, I think, sufficient proof that disorders and diseases are of much longer duration under the homœopathic than under appropriate allopathic treatment, and also, that in acute and serious diseases the mortality of patients homœopathically treated greatly exceeds that of those treated by the ordinary appropriate means."

Dr. Balfour also, who witnessed the Homœopathic Practice in Vienna, says: "Of comparatively trifling cases, many remain for weeks, nay, months, in (Fleischmann's) hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete. . . . Some of the following cases will be found to have been discharged too early, to enable us to be positive as to the ultimate result. . . I have seen one patient refused admittance, and that, too, the very day after his discharge, without any very good obvious reason. It was a boy, with effusion into the right pleura, following scarlatina, which he had gone through at home. There was also a general anasarcous state of the body, which speedily disappeared, but the chief complaint remained obstinate; and, after thirty-three days' treatment with bryonia, the 2d dilution, four times daily, he was dismissed but slightly improved. This boy was denied admission, when he applied the following day on account of return of pain in the chest,—not certainly for want of room, as his bed was empty for days afterwards. This is not the only effusion into the chest which has been dismissed unimproved during the period of my observations; yet this scarcely agrees with Fleischmann's returns, as, out of twelve with exudation in the pleura, occurring during ten years, he has, he says, *cured* all but three, who died. And a physician of the General (Allopathic) hospital has assured me, that many such cases, dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief." (*Dr. Simpson, p.* 109).

It is candid of some of the homœopaths to acknowledge their faults. It is the first step towards a recantation of their errors. Dr. Herring, one of the leading globulists of Philadelphia, in the August number of the 'American Homœopathic Review,' observes, "We take as granted and admitted by the majority of the leading men as an uniform observation made in Germany, as well as in France and England, and here long ago, that in general the success of the homœopaths in our day is inferior to that of the earlier homœopathic practitioners." And on the authority of the 'Lancet,' a "leading homœopath" confesses "we all know that the numbers in our homœopathic ranks are not lessening; but it is the general observation that the number is, year after increasing, who, instead of deriving benefit from homœopathy, are made incurable by so-called homœopathic practitioners." (*Lancet, Dec.* 3, 1859, *p.* 570). We do not see how this can be otherwise. If the homœopathic practitioner gives infinitesimal doses of medicine, such as the billionth of a drop; or, if he faithfully adopt the homœopathic principle and set up in the sick patient an artificial disease to counteract by its power the real disease: in the first case he leaves the patient to the unaided power of Nature—in fact, does nothing at all; and, in the second case he pretends to generate a disease which must be a little stronger than the original one in order to overcome it, and which he does not know the end of. It is gratifying to learn that the more sensible followers of this delusion are beginning to find out their mistakes.

Thirdly.—It is quite evident that almost all the Provincial Hospitals of England and Scotland show a much less death-rate than the Homœopathic Hospitals, and yet admit more dangerous cases,—accidents and surgical operations of all kinds assisting to swell the death-rate. What petty insignificant homœopathic hospital, or the humble homœopathic dispensaries in Leeds, and other large provincial towns, can be compared, for example, with the Leeds Infirmary and the Leeds Dispensary? Let any candid person who *sees* the *outside* even of these humble Homœopathic Dispensaries say whether the two kinds of institutions are to be compared. But let the same candid person examine the *inside* of the Leeds Infirmary and the Leeds Dispensary; let him examine the cases and see their severity and danger, with the accidents frequently brought in, and the capital operations almost weekly performed, involving the risk of life, and swelling the death-rate considerably. Now let this same person ask how many of the patients die, and he will, or can be answered, as far as can be calculated, only about three or four out of every hundred cases—including accidents, operations, patients brought in dying, and cases which were desperate from the first. Next examine the Homœopathic Hospitals. Go into them—examine the *kind* of cases, and you will find them generally mild and insignificant cases. You do not see beds occupied by consumption, paralysis, Bright's disease of the kidney, organic disease of the heart, cancer, &c. But, according to Dr. Gairdner, whose paper we have before quoted, you find more cases of chlorosis, colic, diarrhœa, headache, sore throat, influenza, and chicken-pox, such cases as would seldom or much less frequently be admitted into a large general hospital; and yet what kind of death-rate do they show? According to their own statistics they have a mortality of five or six deaths out of every hundred cases. Now we look upon this as very discreditable, and surely can be no proof of the success of homœopathy, even when we look at mere *numbers*, and much less when we compare the *kind* of cases! Besides, how can you trust that the success which you hear of among the homœopaths is due to pure homœopathy, when you continually hear the advocates of this system say,—"Oh, but Dr. ——— practises *both systems*, the old and the new!" Dr. ——— conscientiously believes that the *old* system is erroneous, but has no objection to violate his conscience now and then when in difficulties! So long as there is nothing to be done in the case but to leave Nature alone, Dr. ——— can practise conscientiously, but when something has to be really done for the patient, he quietly hushes the stings of his conscience, and does the best for his patient on the *old* system,—making use of any remedy which his judgment points out without any reference to homœopathy! Indeed, most of the medicines used by homœopaths have no reference to homœopathy at all, and do not act in the least on homœopathic principles. Hence we condemn the whole thing as a piece of deception.

Let the reader examine the Table on the opposite page, and, (with the exception of the Edinburgh and Glasgow Infirmaries, which we reckon as like Metropolitan Hospitals, and with a high death-rate like the London Hospitals, for the reasons before given), let him notice the low death-rate in the Scotch Provincial Hospitals.

Next let him examine the Table immediately following it, of statistics of thirty English Provincial Hospitals, drawn out by Mr. Thomson, and (with the exception of the large hospital of Manchester, which is rather higher than the rest in its death-rate,) let him observe the small rate of mortality amongst their cases. These tables, we believe, do not include the out-patients; so that if these out-patients were added, it would reduce the mortality in the Leeds Infirmary and Guy's Hospital (as we have shown in a former page) to a remarkably low figure.

Statistics of Eight of the principal Civil Hospitals of Scotland, prepared from the Managers' Reports, and other authorised documents, by J. Thomson, Esq. Showing the number of patients treated; the average residence in hospital; the deaths, and ratio of mortality per cent., with the proportion of deaths to cures. (*Edin. Med. and Surg. Journal*, 1843, *p.* 341,)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Hospitals.	Pop. of respective towns in which the hospitals are situate Census 1841.	Period one year, ending	Under treatment during the year.	No. of cases treated to a termination.	Died.	Cured.	Relieved and discharged.	Discharg. from other causes, viz., by desire, irregular and improp., no benefit.	Average daily number in hospital.	Days in hospital for each cure.	Aver. residence in hospital in days.	Ratio of deaths per cent. of cases terminated.	Ratio of cures per cent of cases terminated.	Proport. of deaths to cures.	Days of residence in hospital for 1 death	Ratio of admission per cent. of the population.
Royal Infirmary, Edinburgh	164451	Oct. 1, 1842	3770	3529	443	2255	551	280	303	41	29	12.55	63.89	1 to 5.09	249	2.3
... Aberdeen	64767	Dec. 31, 1842	1956	1822	85	1458	167	112	113	28	21	4.66	80.02	17.15	481	3.0
... Dumfries	13353	Nov. 11, 1842	508	463	29	352	44	38	41	42	29	6.26	76.02	12.14	516	3.8
Infirmary, Montrose	13811	June 1, 1842	324	295	20	258	7	10	23	32	26	6.77	87·45	12.96	419	2.3
Royal Infirmary, Glasgow..	273147	Dec. 31, 1842	3612	3405	328	2646	226	205	228	31	23	9.63	77.70	8.06	254	1.3
... Dundee ..	63825	June 1, 1842	916	882	45	762	52	23	52	25	21	5.10	86.39	16.93	422	1.4
Northern Infirm., Inverness	11592	Dec. 31, 1842	385	344	15	237	41	41	38	59	36	4.36	69.00	15.80	925	3.3
County and City Infir. Perth	18281	Dec. 31, 1842	486	424	29	305	34	56	42	50	31	6.84	71.93	10.54	538	2.7

Table of the number of patients treated, and the deaths in thirty English Provincial Hospitals, (from material in the British Medical Almanac for 1836 and 1837), showing the average mortality in each. (*Edin. Med. and Surg. Journal*, 1843, *p.* 353.)

Medical Institutions.	Period one year.	Treated.	Died.	Mort. p. cent
Aylesbury General Infirmary,	1835-6,	108	7	6.47
Bath United Hospital,	1835,	814	47	5.77
Bedford General Infirmary,	1835-6,	544	35	6.43
Birmingham General Hospital,	1828-9,	1556	68	4.37
Bristol General Hospital,	1835,	279	13	4.66
Bury St. Edmunds, Suffolk Hospital,	1833,	326	8	2.45
Canterbury Infirmary,	1835,	501	10	2.00
Cambridge Addenbrooke Hospital,	1832-3,	620	17	2.74
Chester General Hospital,	1835-6,	518	21	4.05
Colchester General Hospital,	1834,	284	9	3.16
Derby Infirmary,	1828-9,	584	18	3.09
Exeter Hospital,	1835-6,	1069	35	3.27
Gloucester Infirmary,	1834,	646	33	5.10
Hereford Infirmary,	1828-9,	300	11	3.66
Kingston-upon-Hull General Infirmary,	1835,	528	32	6.06
Leeds General Infirmary,	1832-3,	1298	77	5.93
Manchester General Infirmary,	1833-4,	1852	139	7.50
Newcastle Infirmary,	1828-9,	1100	28	2.54
Norfolk and Norwich Hospital,	1835,	774	46	5.94
Northampton General Infirmary,	1833-4,	756	32	4.23
Nottingham General Hospital,	1835-6,	743	46	6.19
Oxford, Radcliffe Infirmary,	1835,	853	25	2.93
Salisbury Infirmary,	1833-4,	887	27	3.04
Salop Infirmary,	1834-5,	834	30	3.59
Sheffield Infirmary,	1835-6,	894	31	3.46
Stafford Infirmary,	1828-9,	524	13	2,48
Winchester Infirmary,	1833-4,	782	31	3.96
Worcester Infirmary,	1828-9,	680	31	4.55
Suffolk Hospital, Bury St. Edmunds,	1828-9,	250	7	2.80
York County Infirmary,	1835,	305	19	6.22
Total,		21209	946	4.46

Have the public anything to complain of in these statistical tables? Have not their liberal contributions in support of these noble institutions been well and successfully applied in alleviating the miseries of their fellow-creatures, and generally of their poor neighbours? Is it true, therefore, that regular physicians and surgeons are less successful than the practitioners of homœopathy? In this table are included the large hospitals of Bath, Birmingham, Bristol, Leeds, Manchester, and Sheffield, any one of which we suspect contains as many beds as all the homœopathic hospitals inthe whole British empire; and yet the average of deaths is only about 4½ out of every 100 cases admitted, and not even reckoning the out-patients: while the homœopaths acknowledge that they lose nearly six out of every

100. We will not waste any more paper and ink on this subject, as we consider that any candid examiner will feel at once satisfied that we have proved our case.

Fourthly.—We acknowledge that all hospital statistics may be more or less fallacious, but those of military hospitals are less so, as they have been calculated for years, and without any object but truth. Homœopathic statistics have evidently been got up with the object of drawing a comparison between them and others. This is the general impression in the medical profession, and there are many suspicious evidences of this. One suspicious fact we have already related as given by Dr. Balfour, in a preceding table, and a still stronger one we shall have to state when we give the table of cholera cases which occurred at Huddersfield some years ago.

Of all hospital statistics, with respect at least to the death-rate, the Army and Navy Reports must be the most correct. In a general hospital a patient may be dismissed apparently cured, but may die in a month after from a return of the disease; but in the military hospitals every patient is watched to the end of his case, and every death must be accurately known, as all the patients are not only in hospital during sickness but under the eye of the surgeon during health, and for months after dismissal from hospital. It may be said that soldiers are all picked men; so they are, but they are subject to as dangerous diseases as civilians, and have many disadvantages, such as crowded barracks, and sometimes severe work in camp, and in other ways. Moreover it cannot be said that the *families* of the Russian sailors are *picked* men; they are women and children, and yet even in them, when treated by the skilful Russian surgeons, the death-rate in sickness is very low, especially when we consider how many children die in infancy, and what disadvantages the women must labour under in such a climate as Russia. These army and navy returns, both of England and Russia, show the present success of medical treatment by surgeons.

In the Army and Navy Returns, where young and old persons are excluded, the following is the rate of mortality obtained (*Dr. Routh, p.* 45):—

ARMY. 1818—1837.	Admissions.	Deaths.	One in	Per Cent.
Gibraltar	58,227	1,291	45·1	2·2
Malta	44,639	666	70·1	1·4
Ionian Islands	84,438	1,775	47·5	2·1
Bermudas	15,356	338	45·4	2·2
Novia Scotia and Brunswick	36,174	649	56·8	1·8
Cape District	25,506	311	72·0	1·2
Cape Frontier	5,740	65	88·0	1·1
St. Helena	4,360	150	29·0	3·4
Mauritius	38,108	835	45·0	2·1
Canadas	66,957	982	681·0	1·4
Total ...	341,397	8,068	42·0	2·1

NAVY.	Admissions.	Deaths.	One in	Per Cent.
Home Service, 1830—36	25,586	229	111	0·9
Cape	14,858	263	56	1·7
South America, 1837—43	25,361	191	132	0·7
Mediterranean, ,, ...	97,081	996	96	1·6
Variously employed, 1830—36	17,532	171	72	0·9
Total ...	180,418	1850	97	1·0

Here are returns, then, which forcibly contradict the statements that regular surgeons are not so successful as the practitioners of homœopathy.

Compare these English tables with the Russian (*Med. Times, Oct.* 22, 1859, *p.* 401), which, although not quite so successful, are very satisfactory.

Table showing the general number of sick of all branches of the service treated in hospitals, lazarettos, and on board ship :—

Under treatment on Nov. 1, 1857	3201
Cases during the year	45824
Recoveries	43163
Deaths	2120
Transferred to other hospitals	1497
Under treatment on Nov. 1, 1858	2245
Ratio of Recoveries to sick	1·1
,, of Deaths to recoveries	1·20

This is highly creditable to the Russian surgeons of whom we entertain the highest opinion, and when we compare this table with the next, in which the *families* of these Russian sailors are calculated, we are still more pleased, as it shows considerable success among a class of people who must have many disadvantages.

Table showing the sickness in the families of the seamen of the Russian Navy, between Nov. 1, 1857, *and Oct* 31, 1858 :—

Number of cases under treatment on Nov. 1, 1857	183
Number of cases under treatment during the year	6875
Recoveries	6348
Deaths	457
Remaining under treatment on Nov. 1, 1858	253
Ratio of Recoveries to sick	1·1
,, of Deaths to recoveries	1·13

From the above we make it out that the death-rate in the Russian Navy was 4·6, and among the families 6·6; whereas in the British Army and Navy returns, which we have placed before the reader, we find the astonishing fact that in the Army out of 341,397 cases *only about* 2 *out of every* 100 *died,* and in the Navy out of 180,418 *only* 1 *out* *of every* 100 *died!* Now here there can be no deception as to numbers. While the homœopaths *acknowledge* that they lose nearly six out of every 100, we find that the Army and Navy surgeons, not one of whom we ever heard of as being a homœopath, only lose one or two out of every 100, and yet every death must be accurately known.

Even in the Indian army, in a climate and with habits which must greatly aggravate disease, we find the following table *(Dublin Quarterly Journal, Nov.* 1859, *p.* 453*)*, which refers to the Army of Bombay:—

Stations.	Periods of Observation.	Treated per 100 of Strength.	Deaths per 100 of Strength.
Kolapore - - -	20 years ending 1849	300·34	2·05
Sholapore - - -	„ „	226·95	2·22
Kirkee - - -	„ „	177·34	2·64
Belgaum - - -	„ „	172·74	2·75
Punjaub Army - -	„ „	197·82	2·89
Aden - - -	„ „	137·98	2·99
Deesa - - -	„ „	171·82	3·40
Ahmednuggur - -	„ „	233·15	3·45
Bhooj - - -	„ „	209·99	3·83
Poona - - -	„ „	230·33	4·11
Kanark - - -	„ „	100·07	4·37
Mhow - - -	„ „	182·54	4·97
Indus Army - -	„ „	155·56	7·36
Kurrachee - -	„ „	208·43	9·02
Bombay and Colaba -	„ „	254·16	10·92
Hydrabad and Scinde	„ „	369·48	23·52

As nearly as we can calculate, this table shows a death-rate of above 2·7 per cent.

Let the homœopaths dispute these tables if they can. We have given accurate references to the books from which we have taken them. These numerical calculations may be here and there slightly erroneous, but not to our knowledge, and not so as to alter the force of the argument that the homœopaths ought to be ashamed of boasting of their success in treating diseases; and that it is undignified and unworthy of a liberal profession to be circulating their statistical tables in pamphlets and other ways for the sake of drawing comparisons between themselves and regular practitioners of medicine.

Fifthly.—We have now to make a grave charge against homœopathic statistics, which we shall do without giving names, as this might be considered personal. It will be remembered, that some years ago, the cholera prevailed in this country, and amongst other places at Huddersfield. In the 'Homœopathic Times' for 1850 is a report on cholera by a gentleman then practising homœopathy at Huddersfield, but now in a neighbouring town. Well may this gentleman say, "if statistics afford valuable aid in the advancement of science, they are no less useful to those who wilfully essay to pervert facts." Now let us examine this homœopath's statistics, and ascertain whether his object was to advance science or to *pervert facts.* He published the following Table as a Report of the Cases of Cholera which occurred at Huddersfield, and for the accuracy of which he "pledged" himself!

Name.	Age.	Residence.	Treatment.	Results.
William Moss	23	Paddock Head	Allopathic	Died,
Hannah Taylor	55	Ditto ...	Ditto ...	Ditto.
Sarah Tomlinson ...	78	Ditto ...	Ditto ..	Ditto.
A. Micklethwaite ...	17	Ditto ...	Ditto ...	Ditto.
Sarah A. Taylor ...	24	Ditto ...	Ditto ...	Do. (pregnant)
Thomas Dyson	1⅓	Ditto ...	Ditto ...	Ditto.
Jane Dyson	4¾	Ditto ...	Ditto ...	Ditto.
J. Micklethwaite ...	44	Ditto ...	Ditto ...	Ditto.
John Alpin	—	Ditto ...	Ditto ...	Ditto.
Charles Potter	40	Ditto ...	Ditto ...	Ditto.
Jas. Micklethwaite	—	Ditto ...	Ditto ...	Recovered.
Mrs. Micklethwaite	—	Ditto ...	Ditto ...	Do. (supposed cholera).
William Leonard ...	38	Quay-street	Ditto ...	Died.
James Atkinson......	49	Fountain-st.	Ditto ...	Ditto.
Elizabeth Wilson ...	50	Lane ...	Ditto ...	Ditto.
George Jagger	4½	Ditto ...	Ditto ...	Ditto.
Benjamin Berry......	66	Folly Hall	Ditto ...	Ditto.
Mary Goodyear	39	Cropper's-row	Ditto ...	Ditto.
Abraham Smith......	39	Ditto ...	Ditto ...	Ditto.
Eli Wilson	58	Lane ...	Ditto ...	Ditto.
Mrs. Greenhough ...	40	Paddock Head	Homœopathic	Cured.
John Briggs	50	Ditto ...	Ditto ...	Ditto.
John Briggs' Child...	7	Ditto ...	Ditto ...	Ditto.
Thomas Cliffe.........	40	Ditto ...	Ditto ...	Ditto.
Mrs. Richardson ...	35	Marsh ...	Ditto ...	Ditto.
James France..	17	Ditto ...	Ditto ...	Ditto.
Mr. Beavers	35	Sheepridge	Ditto ...	Ditto.
Mrs. F——tt.........	60	Clare Hill	Ditto ...	Ditto.

"Results: number of cases treated allopathically 20; recoveries 2; deaths 18. Number of cases treated homœopathically 8; recoveries 8; deaths 0."

We will now add the remarks of the Reviewer of these Cholera Reports, suppressing the name of the practitioner, for fear of being considered personal. He says:—

"Will our readers credit us when we assure them that this table is a fabrication—ingenious, it is true—but false from beginning to end?

Yet it is so. Few literary members of the profession are ignorant of the zeal, industry, and truthfulness of Dr. John Taylor, formerly professor of clinical medicine in University College, and now a physician in Huddersfield. Dr. Taylor has published a report on the cholera, which is characterized in the Report on Cholera in England lately issued by the registrar-general as 'a model which the local inquirer should consult.' To secure as complete accuracy as possible, he obtained personally the particulars of the cases from the families of the sufferers and the practitioners in attendance; they amounted in all to 93, of which 39, or 40 per cent., were fatal. Dr. Taylor drew up a table of the cases and their termination; and on reference to it he finds, that in or near to the places named in the above table, there were FORTY-SIX RECOVERIES. The homœopath pledges himself (what the pledge is worth may be inferred from that damning fact)—the homœopath 'pledges' himself for the accuracy of his report; yet of 46 recoveries he could only hear of TWO! Was the homœopathist blind and deaf when he inquired as to the fate of John Goodyear the husband, and John Goodyear the son, of Mary Goodyear, whose death he chronicles from 'allopathic' treatment? Could he possibly remain in ignorance, that these two persons were attacked on the day following that on which Mary Goodyear was attacked, and that they both recovered. How did it happen he could state that Mary Goodyear died, but could not say, also, that her husband and son recovered? And these are the cholera statistics of homœopathy! Perhaps the reader may think this *suppressio veri* a solitary instance, and possibly an error. It is neither the one nor the other. The reader will observe that the homœopathist mentions 12 cases of cholera as occurring under 'Allopathic' treatment at Paddock Head, of which, two are reported recoveries, although, with a cunning pretence to great accuracy, he adds the remark, 'supposed cholera,' to one of the recoveries, positively making the mortality 99 per cent. Yet, in reality, there there were 25 cases in that locality, treated by three or four practitioners and by Dr. Taylor himself; the deaths were 11, the recoveries 14, several of the latter being in the same houses as those cases of which the homœopathist recorded the fatal result!" (*Medico-Chirurg. Review, April* 1852, *pp.* 479, 480).

It seems almost incredible that this gentleman should have published such a statistical table, when he must have been aware of numerous other cases of cholera which existed in his neighbourhood, and even in the same family. Although he publishes the death of Mary Goodyear, yet he omits to mention that John Goodyear, the father, and John Goodyear, the son, recovered under regular medical treatment. To show the extraordinary fallacy of this homœopathic report, we append a Table of cases attended by the regular medical practitioners of Huddersfield, drawn up by the late estimable and talented Dr. John Taylor.

Number and Name of Patient.	Age.	Died or Recovered.	Medical Attendant.
1 Francis Donoghue	50	Died.	Mr. Clarke.
2 Mrs. Thomas	38	Recovered.	Mr. Booth.
3 Josh. Whitaker	27	Do.	Mr. Clarke.
4 William Leonard	38	Died.	Mr. Rhodes.
5 Mrs. Hardy	65 (?)	Recovered.	Mr. Tatham.
9 Mrs. Gibson	.. ..	Do.	Mr. Sissons.
7 Mrs. Kershaw	.. ..	Do.	Do.
8 James Atkinson	50	Died.	Mr. Tatham.
9 Grace Bake	29	Do.	Mr. Machill.
10 Mrs. Hirst	53	Do.	Do.
11 Jonathan Booth	.. ..	Recovered.	Mr. Sissons.
12 William Rhodes	45	Do.	Mr. Allatt.
13 Abraham North	35	Do.	Mr. Greenwood.
14 Mrs. Ganter	35 (?)	Do.	Mr. Tatham.
15 John Leonard	.. ..	Do.	Mr. Sissons.
16 Mrs. George Crosland	30	Died.	Mr. Machill.
17 Josh. Thornton	... ..	Recovered.	Mr. Booth.
18 Martha Walker	60	Do.	Mr. Allatt.
19 Mary Craven	35	Died.	Mr. Machill.
20 ——— ——— F.	72	Do.	Dr. Robinson.
21 Grace Bake's Child	1½	Do.	Mr. Booth.
22 Henry Beaumont	27	Do.	Do.
23 ——— ——— M.	70 or more.	Do.	Dr. Robinson.
24 John Stocks	30	Recovered.	Mr. Maxon.
25 Sarah Tomlinson	78	Died.	Mr. Allatt.
26 Josh. Hinchliffe	61	Recovered.	Do.
27 Mrs. Hirst	53	Died.	Do.
28 James Leonard	52	Recovered.	Mr. Sissons.
29 Hannah Taylor	55	Died.	Mr. Allatt.
30 William Moss	23	Do.	Do.
31 John Alpine	40	Do.	Do.
32 Sarah Taylor	24	Do.	Do.
33 Mary Tomlinson	20	Recovered.	Do.
34 Allen Micklethwaite	18	Died.	Do.
35 Hannah Micklethwaite	46	Recovered.	Do.
36 John Micklethwaite	44	Died.	Do.
37 Thomas Wood	45	Recovered.	Mr. Booth.
38 Aaron Taylor	56	Do.	Mr. Allatt.
39 Ezra Whiteley	2	Died.	Do.
40 Charles Potter	29	Do.	Do.
41 Thomas Dyson	1	Do.	Do.
42 Eli Wilson	.. ..	Do.	Mr. Sissons.
43 William Byrom	43	Recovered.	Mr. Allatt.
44 Mary Ann Byrom	7	Do.	Do.
45 Jane Dyson	4	Died.	Do.
46 James Dransfield	14	Recovered.	Do.
47 Sally Byrom	2	Died.	Do.
48 Patrick Connell	26	Recovered.	Mr. Clarke.
49 Rachel Dransfield	18	Do.	Mr. Allatt.
50 Mrs. Kaye	.. ..	Do.	Mr. Sissons.
51 Martha Brown	19	Do.	Mr. Tatham.
52 Ann Taylor	23	Do.	Mr. Allatt.
53 Samuel H. Micklethwaite	21	Do.	Do.
54 J. Taylor	.. ..	Do.	Mr. Greenwood, Mr. Clarke, and Mr. Booth.
55 Ann Byrom	28	Do.	Mr. Allatt.
56 Mrs. Wilson	.. ..	Died.	Mr. Sissons.
57 William Eastwood	39	Recovered.	Mr. Tatham.
58 John Robinson	29	Do.	Mr. Clarke.
59 Mary Ann Ferguson	30	Do.	Mr. Tatham.
60 James Jagger	5	Died.	Mr. Booth.

Number and Name of Patient.	Age.	Died or Recovered.	Medical Attendant.
61 David Taylor	28	Recovered.	Mr. Allatt.
62 Josh. Shepherd	42	Do.	Mr. Tatham.
63 Richard Berry	62	Died.	Do.
64 Mary Goodyear	39	Do.	Mr. Clough.
65 John Goodyear	45	Recovered.	Mr. Clarke.
66 Mary Walshaw	.. ..	Do.	Mr. Sissons.
67 John William Taylor	3	Died.	Mr. Rawcliffe.
68 John Goodyear	5	Recovered.	Mr. Clarke.
69 Robert Whitehead	.. ..	Do.	Mr. Sissons.
70 Harriett Marriott	... ...	Do.	Do.
71 Abraham Smith	39	Died.	Mr. Booth.
72 Mary Quarmby	74	Do.	Mr. Dean.
73 Hannah Pick	17	Recovered.	Mr. Tatham.
74 Margaret Heaton	37	Do.	Do.
75 Maria Moxon	32	Do.	Do.
76 ——— M.	.. ..	Died.	Dr. Robinson.
77 Ruth Berry	.. ..	Recovered.	Mr. Sissons.
78 Emma Wilcock	... ..	Do.	Do.
79 James Charlesworth	61	Do.	Mr. Tatham.
80 John Saville	57	Died.	Dr. Robinson.
81 Josh. Gledhill	60	Do.	Mr. Roberts.
82 ———	18 months.	Do.	Do.
83 Charles Varley	36	Do.	Mr. Dean.
84 Susan Shaw	41	Recovered.	Mr. Tatham.
85 Hannah Clarkson	42	Do.	Do.
86 Mrs. Varley	57	Died.	Mr. Roberts.
87 Mr. Varley	60	Recovered.	Do.
88 Mrs. C. Varley	.. ..	Do.	Do.
89 Mrs. Shaw	.. ..	Do.	Mr. Sissons.
90 Jasper Dawson	36	Do.	Mr. Booth.
91 Richard Thorpe	.. ..	Do.	Mr. Sissons.
92 David Sykes	50	Do.	Mr. Clarke.
93 George William Taylor	2	Died.	Mr. Allatt.

The reader will find a report of *each case* in the 'Medical Times' for 1851, at pages 259, 340, 399, which we believe the Registrar-general considered as a perfect specimen of a medical report. Any one who thinks fit to question the accuracy of these remarks, can now turn to the same papers which we have used, and call them in question if he like.

It thus appears that instead of the regular practitioners having 18 deaths, and 2 recoveries, they had 39 *deaths and* 54 *recoveries!* There can be no deception here. The names are all before the public, with the medical practitioners who attended them. After reading these reports we ask any candid person whether or not he would again trust to the reported statistics of homœopathy. We find that not only is the *severity* of the cases concealed from the public by the homœopaths, but that the actual *number* of cases and deaths are erroneously reported, and therefore we cannot have the least faith in what they say. We honestly confess that we totally disbelieve them in all their statements.

With respect to the statistics of the Vienna Homœopathic hospital, let the reader first remember that this is a private one, in the convent of the Sisters of Charity, and next, that Dr. Fleischmann could admit and discharge the patients without any control. Under these circumstances we do not hesitate to say that any man with any particular object in view could regulate his reports so as to increase their plausibility. Dr. Balfour, who for some time witnessed the practice of this hospital, writes as follows :—

"The patients are admitted and discharged by the physician, without any control, so that, to say the least, it requires a man to be very conscientious to decide impartially between temporary improvement and perfect cure, especially when he recollects that the fate of his creed and his institution may depend upon the nature of his returns to government. These returns are made monthly, with a yearly *resumé.*

"Some of the cases will be found to have been discharged too early to enable us to be positive as to the ultimate result. Again, these cases, or others discharged apparently cured, may apply for readmission, and be, under some pretext or other, refused; while, to disarm suspicion, a few whose relapses seem more manageable may be readmitted. Such may not be the case in point of fact, still it is very possible. I have seen at least one patient refused admittance, and that, too, the very day after his discharge, without any good obvious reason. . . . . . . And a physician of the General Hospital has assured me that many such cases dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief, and which relief they have obtained by the use of purgatives and baths. Then again, there are, I may say, hundreds of trifling cases admitted here, which would not have been admitted into any hospital in England. Many of the patients get no medicine; a few a single dose; and even of comparatively trifling cases many remain for weeks, nay months, in the hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete.

"The whole process of the admission and discharge of patients is mysterious. Still so much is certain, that most of those admitted have been previously visited at their own houses by the assistant. Many cases not improving, or apparently not likely to improve, are got rid of very summarily. During most of the time, I visited in the morning along with Dr. Fleischmann, and latterly, for some weeks, in the afternoon along with his assistant, it not being then permitted to visit in the morning. I was told the cause of this restriction was that the students might have an opportunity of taking a course from the assistant.

"I feel convinced that the secret of Dr. Fleischmann's great seeming success lies in the fact of the admissions and dismissions being entirely uncontrolled, and there being no check on the diagnosis. Rarely other than well-marked cases have their diagnosis written on the board at their bed-head, the others being left blank, and entered in his book, of course as he pleases." (*Brit. and Foreign Medical Review, Oct.* 1846, *p.* 572.)

So that these reports may be as fallacious as the homœopathic cholera reports of Huddersfield, and totally untrustworthy!

"Not only is Fleischmann's hospital at Vienna a private one, but there is no large public hospital in Austria, Prussia, or France, where this system is sanctioned by Government. In answer to notes addressed to the American ministers at Vienna, Berlin, and Paris, the following is an abridgment of the replies:—

"Count Buol—Minister of Foreign affairs in Austria, answers-'1st that in Austria, Homœopathy is taught not by publicly appointed professors, but only by private teachers. 2nd. That this mode o cure is practised, not in public hospitals, but only in cloister, criminal and private hospitals, 3rd. That the private practice of Homœopathy is permitted to any physician who has a Diploma.'

"Ramner, the Prussian Minister, replies, 'I have the honour to inform your Excellency, that Homœopathy in Prussia, is not admitted into the Universities nor Hospitals, nor other public Institutions. Physicians are allowed, if they please, to exercise Homœopathy in private practice.'

Rouland—Minister of Public Instruction in France, writes, 'the exercise of Homœopathy is not legally authorized in France. My administration has not authorized me to exercise any measure having reference to the teaching of Homœopathy.'" (*Brit. American Journal, March* 1860, *p.* 141.)

What will the homœopaths say to Count Buol's assertion that their system of practice is adopted, "not in public hospitals but only in cloister, criminal and private hospitals"?—It must be rather a *let-down* to be thus described by a public minister.

With respect to the statistics of particular diseases, such as Pneumonia, we have as little confidence in them as we have in other homœopathic reports. It is well known that the stethoscope was not used by Dr. Fleischmann in the Vienna hospital, and yet he pretends to distinguish cases of pneumonia. In this way he might put down almost any common cold as a case of pneumonia. Besides, Dr. Balfour detected Dr. Fleischmann in a deliberate attempt to make the report on pneumonia better than it really was. It may seem a trivial fault to miss out a case or two in a medical report, but when the object in view is to deceive the public, it becomes a grave fault, and quite unpardonable. At page 107 of Dr. Simpson's work on Homœopathy, is the following: "There is a striking and instructive episode connected with the averment made by Fleischmann, that in 1846 he only lost *two* patients in his wards from pneumonia. To save the credit of the alleged success of homœopathy in pneumonia, the Editors of the British Journal of Homœopathy, in reviewing the Vienna cases, watched and reported upon Dr. Balfour, and apparently unaware of being preceded in their tactics by Fleischmann himself, attempted to deny that one of the *three* fatal cases seen and recorded by Dr. Balfour was a case of pneumonia. The Editors remark,—

'Dr. Balfour saw nineteen cases of pneumonia. He observes: 'These cases of pneumonia give an average of twenty-four, an average treatment of 12·6 days, and a mortality of 15 per cent.; three out of the nineteen having died. Skoda's cases of pneumonia, during the the same time, amount to forty-five, his deaths to three; giving an average of 6·6 per cent.' This comparison, we think, is incorrect, as we cannot consider Case VI. to have been a case of pneumonia at all; which being omitted, there would be eighteen cases, and two deaths; or 11·1 per cent.' "—(Vol. v. p. 92.)

" Dr. Balfour assures me that this case (No. VI. in his report of the cases of pneumonia) was most unmistakeably marked as an instance of that disease; and was, as already stated written down as such on the patient's board by Fleischmann himself. Dr. Balfour's published report explicitly mentions, that the patient had pain in breathing,—rusty blood-stained expectoration,—the percussion on the right side was dull, as high as the centre of the mammary and scapular spaces,—that over these dull spaces the respiration was bronchial, and elsewhere vesicular, &c.; all of them forming amply conclusive evidence that the disease was pneumonia; and he died on the third day after admission. (*See Dr. Forbes' Review*, vol. xxii. p. 584; and also vol. xxiii. p. 611.) Is it not truly significant, and in itself, portentously suspicious, to find Dr. Fleischmann and his British homœopathic brethren trying,—and both in the same way,—to change the statistics of these pneumonia cases, in order to make them *apparently* conform to the alleged success which homœopathists have had in the treatment of this disease? When we find two writers, independently of each other, thus adopting the same improper tactics, regarding the same cases, does it not show that these tactics are a part of the general system of their medical or statistical creed?"

We need make no comment on this system of deception.

We must say that the homœopaths completely mistake the number of deaths from this disease occurring in regular medical practice. It may be very fatal in some of the large London and Continental hospitals, owing to the severity of the cases when admitted, and the age of the patients, but in general this disease is far from fatal.

We believe that the cases of pure pneumonia are comparatively rare; and when we see numerous cases recorded, we consider that they have been mixed cases, and often complicated with typhus. fatty heart, or valvular disease, &c. No statistics, therefore, of this disease can be correct. You may call a hundred cases admitted into a hospital cases of pneumonia, and perhaps a little pneumonia may be mixed up in the cases. They may all be cured, and given out to be cures of pneumonia, but this is quite untrustworthy, and every candid practitioner, whether of medicine or homœopathy, must acknowledge this to be true. We have enquired at the Leeds Infirmary, and find very few cases of death from pure pneumonia. But the best corroboration of our views is in a paper by Dr. W. T. Gairdner, of Edinburgh, who has taken pains to examine into this subject. During five years Dr. Gairdner lost only ten cases in the Edinburgh Infirmary, although he has generally the charge of fifty beds for different diseases, and "only one could fairly be said to be a death from idiopathic or uncomplicated pneumonia." (*Edin. Med. Journal, March*, 1860, *p.* 834.)

Again, in Dr. Routh's pamphlet we find that, "In 1824, Laennec did not lose one pneumonia case. In 1825, out of 28 pneumonia cases, simple or double, with or without pleurisy, one only died, and this an old man of seventy. In 1825, out of 34 pneumonia cases, 3 died; two females brought *in articulo mortis*, and an old man above seventy. Altogether, Laennec concludes that out 57 pneumonias, he in reality lost but two patients above seventy. Louis lost, out of 20 cases, 3 patients, all from sixty to seventy. In 1831–2, Trousseau lost 2 patients out of 58. None of these were bled in the hospital; but 5 had been so before admission. Since that period, owing to a change of type, he had not been so successful. Professor Bang, out of 54 cases of pneumonia treated at Copenhagen, lost only 2. These were generally bled at the onset. Dr. Wolf did not lose any out of 10 cases (Grisolle). Out of 75 cases treated by Bouillaud, recovery was observed as follows:—55 cases of single pneumonia, 2 died; 16 cases of double, 11 died. Mr. Husson, out of 43 cases, bleeding each patient from one to eleven times, lost only 3 cases. Grisolle collected 44, for the most part severe; in 2 cases the pneumonia was double, 6 only died. The fatal cases occurred among weakened individuals, who were generally above fifty years old. Were I to imitate in this compilation the homœopaths, I could conclude I had collected some 300 cases, with a mortality of about 3 per cent. In justice, however, such a computation and comparison must be condemned; the type and peculiar treatment of each being so different, and admitting of no general conclusion" (*Dr. Routh, p.* 57).

There can be no doubt, however, about the value and accuracy of the Army and Navy Reports. "The official Army Returns show 12,271 cases of pneumonia among the British troops stationed at Gibraltar, Malta, Ionian Islands, Bermudas, Canada, Cape, Mauritius, and St. Helena. Out of these 12,271 patients attacked with inflammation of the lungs, 413 died; or, in other words, 1 in every 29 cases of the disease was fatal; or, about 3 in every 100. Out of above 2000 cases of pneumonia at Gibraltar, only 1 in 45 proved fatal; or, about 2 in every 100 attacked with this disease.

The official Navy Returns show 3,099 cases of pneumonia among the sailors in our fleets when stationed at the Cape, in South America, the Mediterranean, &c. Out of these 3,099 patients attacked with inflammation of the lungs, 136 died; or, in other words, 1 in every 23 cases of the disease was fatal; or, about 4 in every 100. Out of 295 cases of pneumonia in the ships placed on the South American stations, only 1 in 59 died; or, less than 2 in every 100 attacked with this disease died." (*Dr. Simpson, p.* 113.)

In conclusion the most charitable opinion to arrive at with respect to the fallacies of homœopathy, is that the writers on this system become muddled, and at last begin to think that their fallacies *are* truths. Some will write in such a way that each word is correct enough, and perhaps each sentence, but somehow when you have read the whole paragraph or speech, you can make neither *head nor tail* of the subject. For example, M. Granier, a celebrated homœopath, writes as follows :—" I would ask if in the middle of the 19th century, an age eminently favourable to the development of *fluidism*—any one should object to consider diseases as the *fluidic derangements of our vital fluid?* which is perhaps our *specific electric fluid.*

" Medicines are *fluidic powers.* They are *beings* that man may create at his will. I wish I could say they are occult powers forming the chain of fluidic communication between this world and that beyond the tomb. But I am convinced in my own mind, that placed on the limits of *fluidic dynamism*, our observation might cast a scrutinizing glance into the unseen world" (Conferences on Homœopathy by M. Granier). (*Lancet, Feb.* 11, 1860, *p.* 148.)

The works of Hahnemann are nonsensical enough, but M. Granier is a recent writer, and one who is supposed to be *well up* in all the modern improvements of homœopathy.

It has often amused us to see the difference of opinion amongst homœopaths: take for example the opinion of this said M. Granier respecting camphor. Every one who has examined homœopathic medicines and tasted them (such as can be tasted) will see how often *camphor* is prescribed. But, says Mr. Granier, the very smell of camphor "forms an antidote to homœopathic medicines." He calls it the "*oidium* of the pharmaceutical vineyard," and declares that a piece in a patient's room destroys the influence of his remedies. This may account for the want of success of some homœopaths, who, we know from our own personal knowledge, have been giving strong doses of spirits of camphor with a solution of morphia when the bowels have been blocked up for several days, and no attempt made to relieve them.

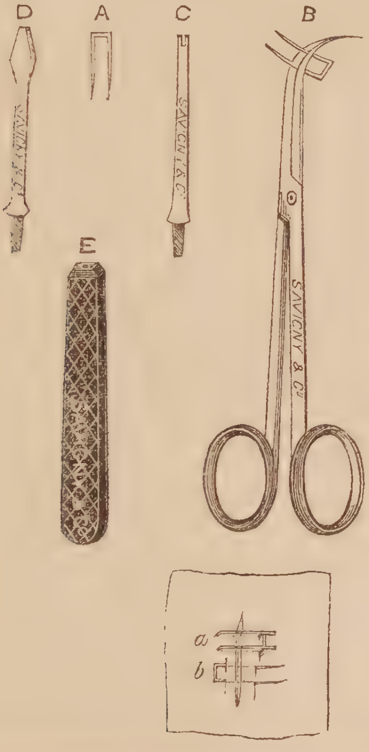
We think that we have now in these three pamphlets written enough to prove that the practice of homœopathy in the selection and doses of their medicines is absurd—that its principles are unscientific and untenable—and that its statistics are untrustworthy. We shall hereafter decline troubling ourselves further on this subject.

135.—DESCRIPTION OF THE NEW STAPLE SUTURE WITHOUT A LIGATURE.

By James Startin, Esq., F.R.C.S., Surgeon to the Hospital for Diseases of the Skin.

[The following method of uniting wounds has been lately adopted by Mr. Startin in some operations for the removal of scars on the face and neck, and also in fissures of the soft palate and hare-lip.]

A glance at the annexed diagrams will at once render evident the nature and the simplicity of the apparatus :—



The above diagrams are about a fourth of the ordinary size.

A is what may be called the Staple Needle, represented smaller and thicker than the ordinary size, though this, of course, can be varied according to the case. It is made of No. 30 steel wire, ground to a triangular or glover's point, and, with the exception of this point, afterwards softened so as to render the shank of the needle as flexible as iron wire.

B is a pair of curved forceps, with the exception of the curvature, resembling the common dressing forceps, the bent ends of which are made circular, and coated with lead, so as to meet each other by their circumferences only, and grasp firmly the staple needle, previously bent into the required curve, just above its hard or steel portion (see diagram), so as to push it through the lip or lips of the wound to be united. After the points of the needle have passed out through the skin, the forceps may be again employed to draw it into the proper position and adjust it for the fastener.

C, D, E, are the fasteners and handle ; E consists of a handle into which C or D may be screwed ; C is a firm piece of iron-wire, in the end of which a narrow slit or fissure is formed to the extent of the eighth of an inch ; which slit, to prevent the needle's point slipping, is tinned or leaded ; one or both points of the staple needle being inserted into this slit, and the fastener being carried as close as possible to the needle's place of exit from the skin, half a revolution of the instrument by the hand fastens the suture neatly, and turns one or both of the protruding points or portions of the needle over the external wound, thus keeping its edges in apposition, and preventing their everting. If, as is most convenient, one protruding needle-point was only been acted upon, the other point or portion will require to be fixed in like manner ; D, the other fastener, made to screw into the same handle, is adapted for the cavities where C cannot be conveniently used, as the roof of the mouth, for example ; it is made by doubling a piece of soft metal into the shape indicated, so as to form a sort of large eye through which the points of the staple needle are severally passed, when a half turn of the handle doubles the needle over the wound, as described as being accomplished by the other fastener ; this form of fastener, when it has been pushed as close as possible up to the point of exit of the needle from the skin or mucous membrane, enables the operator to exert sufficient traction upon it to close the wound, and thus assist the action of the curved forceps. A pair of scissors may be used to cut off the points of the staple needle, if required, and a little pressure with a spatula or the handle of the fastener will adjust the wires so as to make them lie evenly and smoothly over the wound.

a, *b*, Diagram showing the staple suture applied. *a*, The completed suture. *b*, The suture before using the fasteners.

To remove the Staple Suture when required, the fastener D, or C, must be used to turn the wires which pass over the wound in a contrary direction to that which fastened them : this procedure will render the staple straight again, and enable it to be withdrawn by a pair of hooked forceps or a small hook passed under the transverse or curved end of the staple.

The advantages of this needle appear to me to be, firstly, its comparatively easy application; secondly, the absence of a ligature, whether of silk or wire, to disturb or irritate the wound in drawing it through the skin, with the subsequent trouble of knotting it, etc.; thirdly, the little pain attending the application of the staple needle, and the firmer and less-puckered stitch made, owing to the shape of the needle, forming as it were, a connected suture; and, fourthly, the action of the bent portion of the needle over the wound in forming a substitute for the shield, threads, etc. in ordinary use for keeping the external edges of the wound in apposition.

In very large fistulous wounds a single firm ligature may, in the first place, be advantageously passed through the centre of the fissure before the staple suture is employed; and this may be conveniently done by the tubular needle I have before described in your pages, and which has been adopted by my friends, Professor Simpson, Mr. Spencer Wells, Mr. Paget, Mr. Curling, and others; the stitch being fastened by the leaden clamp I invented with and for this needle, and which has since been described by Mr. Baker Brown in the pages of the 'Lancet.'—*Med. Times and Gazette, April* 21, 1860, *p.* 391.

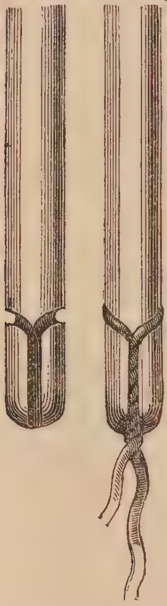
136.—*Iodine Injections in Fistula in Ano.* By M. Bonnafont.—In the more recent cases of fistula in which the revival of the old, indurated edges of the aperture by means of the cutting instrument has not yet become necessary, iodine injections may lead to a rapid and durable cure. One of the greatest objections to this plan, however, is the severity of pain caused by the iodine coming into contact with the mucous membrane of the rectum,—pain which sometimes radiates with violence over all the neighbouring parts, and gives rise to abundant diarrhœa. M. Bonnafont, of the Invalides, adopts the following procedure in order to mitigate this inconvenience. The introduction of a tent of charpie or lint passed into the rectum, often enables us to determine whether a fistula is complete or incomplete, as in the former case when we inject a coloured fluid, as ink, milk, or litmus into the external opening of the fistula, the tent imbibes some of this fluid, proving beyond all doubt the completeness of the fistula, even when a probe has not been able to be passed through the internal aperture. After having by this means ascertained the fact and the distance of the internal aperture, M. Bonnafont passes a good-sized tent into the rectum and carries it to about two centimetres above the internal orifice of the fistula. When the iodine injection is thrown into the fistula, any of it which passes through the aperture is absorbed by the tent, and thus prevented spreading within the intestinal canal; and its irritating effects are thus greatly mitigated.—*Union Méd.*—*Med. Times and Gazette, April* 21, 1860, *p.* 401.

137.—NEEDLE FOR THE WIRE SUTURE.

By R. J. Levis, M.D., Surgeon to the Philadelphia Hospital.

The present general use of the metallic suture in surgery will make acceptable a form of needle adapted to its peculiarities, and which, by facilitating its introduction through the tissues, will add to its convenience and efficiency.

For the free passage of the needle and wire, it is essential, first, that the wire be securely held; second, that it present, at its connection with the needle, no impediment to the transit through the tissues; third, that it should follow the needle in a direct line, not allowing an angle to form at the junction which will require traction to overcome. It is also important, for the convenience of the operator, that the needle should be readily threaded.



In using the ordinary surgical needle for the purpose, even though the needle be deeply grooved at the eye for its lodgment, the wire forms a ring-like attachment with the needle, which impedes its passage. A sort of hinge-joint is also formed at the junction, which will be moveable, no matter how tightly the wire be twisted, and there will be continually forming an angle with the needle, which is a great impediment to its use in delicate tissues, and in regions difficult of access, as the vagina, rectum, and fauces. Another inconvenience which has occurred with me in the use of the ordinary needle and the silver wire, is the liability of the wire to be pinched off by the forceps necessary to its introduction in the above localities.

These objections have induced some surgeons to introduce the wire by first passing the ordinary silk thread to which the wire is attached, and then drawn through.

For the purpose of overcoming these inconveniences in the ordinary use of the wire suture, I have devised a modification of the needle, which is so well illustrated by the accompanying proportionally colossal representations, as to render description almost unnecssary.

Its peculiarity is in what is usually the eye of a needle, though this is really without an eye, the attachment of the wire being accomplished by grooves, in which it rests.

A groove, deep enough for the lodgment of the wire, encircles the needle obliquely near its extremity, and leads into another groove, which is vertical. The vertical groove is just wide enough at its entrance to admit the introduction of one wire at a time; but the inside of the groove being large enough to accommodate two wires, when both are introduced and twisted together they are securely held. An attachment is thus effected which is as firm as if the needle and wire formed one continuous piece. and the wire being entirely incased within the grooves it will traverse any tissue of the body without the least impediment.

There is a decided advantage in having the wire double for an inch or more following the needle, as any break in the wire invariably occurs very near to the needle.

This form of needle has been so extensively used in this city as to thoroughly test its efficiency, and it has been preferred by the instrument makers on account of its simplicity and the facility and cheapness with which it can be made.

Such needles, of sizes adapted to all uses, may be had from Mr Gemrig, of Eighth Street, or Mr. Kolbé, of Ninth Street, Philaphia.—(*Med. and Surg. Reporter, Dec.* 3, 1859, *p.* 224.)—*North American Review, Jan.* 1860, *p.* 176.

138.—GANGLIONS OF THE WRIST CURED BY SUBCUTANEOUS DIVISION, FOLLOWED BY COMPRESSION.

Henry H., seventeen years old, by profession a barber, presented himself at the clinic of the Jefferson college, Philadelphia, in November, 1859, on account of two small, ovoidal tumors on the extensor side of the wrist, which interfered somewhat with the motion of the joint, and the exercise of his profession.

The tumors, which had begun to form about seven months ago, were of a round shape, about the size of a marble, painless, unaccompanied by any discoloration of the skin, elastic, and movable. The patient could not remember having sprained or bruised the part at any time previous to their appearance. Professor Gross diagnosticated them to be *ganglions;* that is, small encysted tumors formed in the course of one of the extensor tendons.

In the *treatment* of the case a delicate tenotome was introduced subcutaneously, and the cysts cut up as minutely as possible. On puncturing the tumors, a yellowish, jelly-like substance escaped, and they became completely effaced. After the operation compression was made with a compress and piece of coin, confined by a bandage, and the patient ordered to keep his arm at rest in an elevated position, and to restrict himself to a light diet. The local inflammation following the operation was but slight, and soon subsided. Under the external application of tincture of iodine, in conjunction with the treatment here mentioned, the cure was completed within a few days, no trace of the swellings being left.

Remarks.—Professor Gross is inclined to ascribe the origin of tumors of this kind to inflammatory action, having observed them most commonly in hard-working people. He assumes that they are merely sacculated expansions of the sheath of the tendons, and not a new formation, as some pathologists suppose. The sac consists of a single layer, rarely thicker than the dura mater. While a common mucous burse usually secretes a fluid like ordinary synovia, a ganglion contains a yellowish, viscid, and transparent fluid, resembling white of egg, or, as in the case reported above, jelly; in old cases the contents are sometimes more solid, and consist of masses of semi-organized lymph, or small fibroid bodies, usually of the size and shape of cucumber-seeds. In a ganglion of the hand, upon which Professor Gross operated a few years ago, he observed these bodies in a variety of singular shapes—round, oval, and spindle-shaped, with a number of prolongations; they were evidently merely masses of organized lymph, which had been originally adherent to the walls of the sac, and had become detached in consequence of the friction of the tendon against the sac. Ganglions are most liable to form on the extensor tendons of the hand or wrist, although they are occasionally met with in other parts of the body. Professor Gross operated, in 1854, before the medical class of the University of Lousville, upon a young woman who had two tumors of this kind, each of the size of a small bird's egg, upon the dorsal surface of the foot. There are cases on record—and one of them has come under Professor Gross's own observation—where a ganglion was situated over the radial artery, and was from the pulsation communicated to it, at first supposed to be an aneurism. Ganglions can be but seldom cured by stimulating applications; although the external use of iodine, and systematic compression may, in recent cases, sometimes produce absorption, and lessen the size of the tumor, the fluid in the cyst generally accumulates again. The best and most simple plan is to make firm pressure upon the sac, so as to rupture it, and allow the contents to escape into the surrounding cellular tissue; absorption will then take place, and may be rendered more speedy by the application of iodine; after the evacuation of the cyst, a compress and bandage should be applied to the part, to promote the union of the opposite side of the cavity by adhesive inflammation. In recent cases the rupture of the sac can be effected either by pressure of the thumb, or by striking the tumor with a large book; in cases of long standing, where the wall of the cyst is thicker and firmer, it is best to make a subcutaneous puncture. If the ganglion resists these measures, it is most effectually destroyed by introducing a bistoury subcutaneously, cutting it up into small pieces, and afterwords applying compression. It has been recommended by some surgeons to treat these cysts after the manner of hydrocele, by introducing a seton, or by injecting iodine; but the success which has attended this method does not speak much in its favor. As a general rule, free incision into the tumor, or excision of it, should be avoided, as it is liable to give rise to a high degree of inflammation, the results of which might endanger the use of the joint; still there are cases on record in which extirpation has been successfully practised, and this plan of treatment has the support of old authorities, such as Celsus and Paul of Ægineta.—*North American Review, Jan.* 1860, *p.* 82.

139.—ARGENTI NITRAS FUSUS CONTAINING CHLORIDE OF SILVER.

This new form of lunar caustic is prepared by adding a definite proportion of hydrochloric acid to the crystals of nitrate of silver at the time of fusing, and pouring the fused mixture into moulds in the usual way. The proportion of the officinal acid which gives five per cent. of chlorid in the fused nitrate, is forty grains to two ounces of the crystals.

The sticks of lunar caustic thus made are of a darker colour than usual, in consequence of the more rapid effect of light upon the chlorid. They are harder, much less soluble, and of about double the strength or toughness of the pure nitrate. When fused nitrate of silver is perfectly pure, the sticks are grayish white, translucent, of a crystalline structure, and are very brittle—almost friable, and so soluble, that, in application to moist surfaces, more of the salt is left upon the tissue than is desirable.

The increased strength of the sticks in this new form, and the diminished solubility must therefore prove to be very valuable qualities in a large proportion of cases where the nitrate is used. While as yet there are no known disadvantages from the admixture.

This improvement in lunar caustic is due to Prof. J. Lawrence Smith, of the Medical Department of the University of Louisville, Ky., and an account of it was published in the 'Semi-Monthly Medical News,' of Louisville, for Feb. 1859, p. 65.

It is known in the commerce of lunar caustic, but not so well known in the medical profession, where this important article is used, that the manufacturers without any known exception sell two kinds, which are of different degrees of purity, and which are often designated on prices current as "No. 1" and "No. 2," or as "Pure" in the one case, and "No. 2" in the other. The "No. 1," or so-called "pure" nitrate, commonly contains from 1 to 12 per cent. of nitrate of potassa, or other cheap salt added, as is alleged, for the purpose of toughening the sticks, and to make it run well in the moulds. The "No. 2" commonly contains from 25 to 33 per cent. of the cheaper adulterating salt. In "the trade" these two varieties are known and distinguished by the sticks being rolled in one case in white paper, and in the other in white paper of a blue tint—or, when both are rolled in paper of the same colour by the ends being sealed by wax of different colours. And when in half pound or pound bottles the figure "2," often in lead pencil, may be found in one corner of the label. These delicate distinctions do not, however, often accompany the article into the hands of medical men, and the effect is so unmistakably bad that the writer, some time since, devoted much attention to the discovery of some simple easy test whereby the fraud might be detected. In this he was fortunately successful, and the test as now exhibited to the Society will easily and effectually discriminate between a good and bad article, and, if relied upon, will probably cause four-fifths of the lunar caustic in the hands of medical men to be rejected, taking the profession at large. A small fragment of nitrate of silver—say four times the size of a pin's head—crushed to powder with a knife-blade upon a piece of paper, the powder spread out over the paper, and the paper and powder then rolled up into a small match-like roll, set on fire, and burned, leaves a tasteless residue of pure silver. But if the nitrate contains even 1 per cent. of any saline impurity the residue, instead of being tasteless, will have the sharp alkaline taste of the base of the adulterating salt. The little match burns rapidly with deflagration, and provided the quantity of the nitrate taken be not too great for the size of the paper, and provided the powder be well distributed through the paper, the carbon of the paper will in burning reduce perfectly all the silver, and in great measure the base of the adulterating salt also.

With this simple test no one need be deceived in the character of the lunar caustic he uses, until some new and more fixed adulterating substances are found. The chlorid of silver in the new form does not interfere at all with the application of this test, since in that case also the residue is tasteless.—*New York Journal of Med., March* 1860 *p.* 165.

140.—*Treatment of Tetanus by injection of Tartar Emetic.*—[At a meeting of the Medical Society of London, when the treatment of tetanus by wourali was under discussion, Dr. Mackenzie stated that]

Mr. Ellis, in the nineteenth volume of the "Medical Commentaries," had related a case of the disease treated by the injection in large quantities of tartar emetic, two or three successive injections, containing a drachm each, having been used at intervals of four hours; the tetanic symptoms were completely removed, and the patient made a good recovery. Dr. Mackenzie then referred to a case which had come under his own care at the Western Dispensary, and in which he had employed the same treatment, with the exception that he had added opium to the tartar emetic. The tetanic spasms much diminished, but the patient eventually sank from exhaustion. The tartar emetic seemed to develop the sedative powers of the opium in a marked degree.—*Lancet, Nov.* 5, 1859, *p.* 469.

141.—*Use of the Castor Oil Plant as a Galactagogue.*—Dr. Routh exhibited three preparations of castor oil leaves, a tincture and a liquor (dose of each, one drachm), and an extract (dose, five grains).

The leaves were obtained from Australia; and the drugs had been prepared by Mr. Greenish, of New Street, Dorset Square, where they could be procured. Dr. Routh had read a paper on the subject of the galactagogue effects of this plant, the leaves of which, applied to the breasts as poultices, and as fomentations to the vulva, for three days at intervals, were used in Boa Vista, to induce milk in the breasts of any woman within catamenial ages, but particularly if these women had borne children, to produce milk. The milk, once produced, could be perpetuated by the simple irritation produced at the nipple by the suction of a child. These facts, related by Dr. WcWilliam, had been confirmed in part by Dr. Tyler Smith. Dr. Routh had published his experience on the subject in a series of papers in the *Medical Times and Gazette* of this year. To lying-in women with a deficiency of milk, he had given the infusion, in combination with conger-eel soup; and the effect in determining a copious flow of milk had been remarkable. He had administered the extract to unmarried women within catamenial ages, and the effect had been to produce intense pain in the breasts; but, as he could not find anybody, in that case, who would try the effect of a child, he had not yet induced milk in the breasts of such; but, after three or four days, the symptoms were relieved by a copious leucorrhœa. As it was possible that a larger experience of this remedy might enable us to convert some married women (within catamenial ages) into wet nurses, and as it undoubtedly acted as a powerful lactagogue in suckling women, he was anxious that others also should experiment, and therefore to direct them where it could be procured.—*British Med. Journal, Dec.* 17, 1859, *p.* 1,030.

142.—ON THE ACTION OF SOME OF OUR ORDINARY MEDICINES.

By C. Handfield Jones, M.B., F.R.S., Physician to St. Mary's Hospital.

There are few drugs (would that they were more numerous) which a large experience justifies us as endued with the power of arresting *sthenic* inflammation, when set up in certain tissues. Thus *antimony* arrests pulmonic and renal inflammation; *mercury*, iritis and peritonitis; *chlorate of potass*, buccal inflammation; *iodide of potassium*, periosteal; *colchicum*, synovial. Such remedies I have proposed to term tissue-sedatives, believing that, whatever physiological action they may have, their pecular efficacy in disease is quite independent and apart from it. Our best knowledge respecting colchicum (Garrod), respecting antimony (Ackerman, 'Brit. and Foreign Medical Review,' April, 1859), and respecting iodide of potassium or chlorate of potass, does not explain to us in any measure the cause of their acting on certain parts and not on others, or how they produce a cessation of the inflammatory process.

Alkalies, including their vegetable salts, it seems probable to me, belong also to this class. They are efficacious in some cutaneous inflammations, as psoriasis; in some inflammations of the urinary passages, some bronchial and laryngeal inflammations, and in acute rheumatism. In the latter disease, it has been presumod that their beneficial effect depends on their neutralising the excessive acidity of the system. This, however. seems very questionable—(1) because it is not at all proved that there is any excessive acidity or diminished alkalinity of the blood; (2) because the alkalies fail to benefit in cases of rheumatism of asthenic and apyretic character; (3) because other remedies, of a totally different kind, as colchicum and aconite, occasionally cure acute rheumatism; (4) because alkalies are of no avail in the visceral inflammations of rheumatism; (5) because neutral salts, as the nitrate and acetate of potash, are curative of rheumatism, the former of which can exert no alkaline effect, while the decomposition which ensues of the acid of the latter proves that the system in acute rheumatism still retains its power of assimilating or digesting acid, and, therefore, that it is reasonable to suppose that it would also digest lactic acid, the presumed *materies morbi.*

Lead, in the form of a strong Goulard lotion, has so marked a sedative effect on an inflamed cutaneous surface, that it seems to me to belong to the class of tissue-sedatives as much as to that of astringents. It is certainly a more sedative application than tannin, but, like it, exerts also an astringent action on the capillaries, which renders it efficacious in hemorrhages not depending on actual ulceration, and in serous profluvia.

Another class of medicines may be distinguished of an opposite kind to the preceding. These are the nerve-tonics. Among them I include quinine, arsenic, strychnine, digitalis, and to some extent, iron. Their action, so far as we trace it, is not eliminant in any degree, nor depressing, but invigorating. We conclude, that the nervous system is mainly influenced by these remedies, because they seem to operate much in the same way as exciting and invigorating mental influences, which can only act through the nervous system; because, while there is an evident resemblance of therapeutic effect in all, two of them (quinine and strychnine) do operate most undeniably on the nervous system, and because they are all found beneficial in disorders whose chief character is depression of nervous power. If we consider the above-mentioned drugs separately, we shall, however, see that they do not all act alike.

Quinine seems to exert its influence both on the cerebro-spinal, and visceral sympathetic nerves, it slows the heart's movement, and prevents the hyperæmic affluxes that take place in malarious disease from vaso-motor nerve paralysis. In this latter case, as well as in the case of neuralgia, the beneficial effect probably depends on the nervous tissue being rendered more vigorous and strong, more able to resist injurious influences. On the same principle we must explain the protection conferred upon the system by the use of quinine in respect of malarious miasms. The protected system resists a poison it would otherwise succumb to. Such facts as these show positively that quinine is not an antiperiodic merely, as some are in the habit of calling it.

Arsenic, in many respects, has a similar action to quinine. Its efficacy in malarious disease has long been known, and has lately been especially tested by M. Boudin, in Algeria. In various neuralgiæ, it renders good service, particularly in obstinate sciatica, I have much reason to be satisfied with it. It exercises a very marked influence over the vaso-motor nerves of the skin in the cure of eczema and other vesicular eruptions, over the intestinal in some cases of chronic diarrhœa, and over the uterine menorrhagia. It acts as a tonic to the musculo-motor nerves in chorea, and to the bronchial in some cases of asthma, rendering them less liable to be thrown into spasmodic action. It is a remedy of much slower operation than quinine, and much more liable to cause tissue irritation. It is therefore especially important if we employ it in inflammations, to be sure that the morbid action has lost its sthenic character.

Strychnine is notorious for its especial action upon the musculo-motor apparatus, affecting particularly the spinal cord. Professor Kölliker asserts it has no action upon the brain, but I am strongly disposed to question this statement from observation of its therapeutical effects. Trousseau and Bardsley both speak positively of its curative influence in cases of hemiplegia, and I have recently had myself a well-marked instance of recovery from facial paralysis under its use. In some cases of amaurosis, there seems no reason to doubt that strychnia has proved curative. In many cases where it has failed, it is very probable that the condition was incorrectly diagnosed, and that there existed grave structural lesions. From some personal experience of it, I should say that it acted certainly as a cerebral tonic, invigorating the hemispherical ganglia. In chorea, it has often a very beneficial effect, steadying and invigorating the motor nerve apparatus. Its action on the sympathetic is demonstrated by its effect in arresting some forms of vomiting and chronic diarrhœa, as well as passive menorrhagia, and (as discovered by Dr. Vernon) in exciting the gravid uterus when inert to vigorous contraction. Its beneficial effect in ague and neuralgia has been observed by Mr. Wilkinson, and is quite in consonance with the view of its acting as a general tonic of nerve-centres, not only of the spinal.

Iron, in some of the various forms in which we administer it, has manifestly a nerve-toning and steadying action. Carbonate of iron is an approved remedy in neuralgia. Tincture of muriate of iron positively asthenic hyperæmias and inflammations, in fact, such controls states as we may fairly suppose to involve a paralysed condition of the vasomotor nerves, with languid tissue power. I can confirm, from my own experience, at least to some extent, the favourable reports given by Mr. Bell, of the excellent effects produced by tincture of sesquichloride of iron in erysipelas and desquamative nephritis. The analogous pernitrate salt has a similar effect, according to Graves, in some cases of chronic diarrhœa. The efficacy of tincture of sesquichloride of iron in spasmodic stricture of the urethra is a remarkable fact, illustrating the special affinity of particular remedies for particular systems of organs, and the control of a state of spasmodic contraction by an agent which is neither contra-stimulant nor sedative. Recently, I have observed a remarkable instance of most severe nocturnal spasm of the muscles of the back and loins, which was markedly controlled by the free administration of quinine and stimulants. Such instances show that in many cases it is quite possible for muscular spasm, as well as neuralgic pain, to depend *au fond* on a feeble and consequently over excitable state of the nerve or nerves involved.

The knowledge we possess of the action of *digitalis* seems to justify us in ranking it as a nervine tonic. It slows the action of a weak heart, and at the same time strengthens its contractions, so that the pulse increases in force. It acts in the like manner with the uterine blood-vessels in passive menorrhagia. It stimulates the renal tissue to increased activity. In all these therapeutic actions, it appears as a stimulant or tonic. But it may be used also to diminish the excessive action of the heart in inflammatory fever and hypertrophy. In this case, at the same time that it slows the action of the heart, it renders the pulse feeble, and often irregular, and syncope is imminent. It is probable that in this case the arrest of action depends upon an inhibitory effect, the excessive stimulus producing an opposite effect to that occasioned by a moderate one. The result of Mr. Lister's experience is to the effect "that in a healthy state of the nervous system very gentle irritation of the vagus increases the heart's action, while a slightly stronger application diminishes the frequency and force of its contractions." The following quotation, from the same highly interesting paper, bears precisely upon this point. "When partial exhaustion has occurred, a much stronger galvanic stimulus is required to produce the same effect upon the heart, than at the commencement of an experiment; and thus an action of the battery which, when first applied, causes marked diminution in the number of beats, may after a while come to have the opposite effect, and increase the heart's action as decidedly as it had previously lowered it; while at an intermediate period it may seem to have no influence at all." Just in the same way digitalis tones and strengthens the action of a feeble heart, but lowers that of a vigorous one.

The only narcotic remedy that it seems necessary to consider with regard to the therepautics of fever and inflammation, is *opium*. One remarkable circumstance is the extraordinary tolerance evinced by the system under such disease of large doses of this drug. Patients, in rheumatic fever, will take as much as twenty-four grains in the twenty-four hours, without any notable result. In puerperal peritonitis, the same quantity has been administered by Dr. Churchill, with the best effects. Its action on the cerebro-spinal nervous system is evident in the calmer and quieter condition of the sufferers, it renders the sensorium less impressionable, and the system less liable to be exhausted by the disease. Its action on the sympathetic and vaso-motor nerves is probably similar, it will relax contracted arteries, and admit a freer transit of the blood. This is probably the *rationale* of its beneficial effects in the cold stage of ague, and in similar conditions; it does away with the contraction of the superficial vessels and skin. In many choleraic attacks which have much resemblance at first to an attack of ague, a dose of opium and chloric ether is of the greatest efficacy. So, in exposure to severe cold and in gangræna senilis, the beneficial effect of opium probably depends on its preventing the arrest of the circulation in the limbs and superficial parts, by keeping the arteries, especially the smaller vessels, relaxed and patent. At the same time it protects the heart from the depressing inhibitory influence of the cold, and so enables the circulation to be well sustained. The latter remark will also apply to the case of peritonitis, and of the shock from burns. The sudorific effect of opium probably depends also on its relaxing the cutaneous arteries. In sthenic inflammation, we have no evidence to show that opium, *per se*, is of any avail. But in asthenic, it certainly seems to exert power in controlling exudation. What else can be said of the action of this drug in diarrhœa, but that it arrests the outflow of watery fluid through the mucous membrane? In common nasal catarrh, it has in some persons the same effect. Its "locking up" the secretions of the liver and other glands is an action of the same kind. In all such cases we may presume that the drug affects the tissue of the part in a sedative manner, analogous, if not identical, with its action on the nervous tissue or the arterial contractile. It is therefore a tissue sedative. If the action of the heart be in danger of failing from asthenia, the administration of opium in my opinion is unsafe from this very sedative effect. Yet where the depression is the result of violent irritation in another part, as for instance, in peritonitis, the action of the heart may greatly improve under the use of opium. The narcotic, by rendering the cardiac ganglia less impressionable, takes of the inhibitory effect of the peritoneal irritation.—*Brit. Medical Journal Dec.* 17, 1859, *p.* 1018.

143.—ON OXYGEN AS A THERAPEUTIC AGENT.

By S. B. Birch, M.D.

[The author, after a few preliminary remarks, passes on to give a brief outline of the main results hitherto obtained from experimental research on the above subject.]

Dr. Gairdner's experiments on rabbits go to prove that the inhalation of pure oxygen increases the fibrine, and diminishes the corpuscles and albumen of the blood.

Dr. B. W. Richardson, following out this idea, in his admirable work on the *Cause of the Coagulation of the Blood*, details some experiments proving that a condition of hyperinosis, with tough fibrinous concretions in the blood, is induced by prolonged inhalation of pure oxygen.

Mr. Savory, *On Animal Heat*, states that he has found the temperature to be lowered, not increased, by an extra amount of oxygen. My own experience, however, tends to the conclusion that in healthy well-fed animals the animal heat is usually first increased, then lowered; while clinical observation has afforded satisfactory evidence that oxygen, when employed in disease, will raise or lower the temperature *under different circumstances.*

Lastly, I would draw special attention to the highly interesting experiments of Mr. Erichsen, detailed in his monograph on *Asphyxia.* He says :—"In a considerable number of experiments that I have performed on this subject, I have never succeeded in re-exciting the contractions of the ventricles by means of the inflation of the lungs with common air, provided they had fairly ceased to act before artificial respiration was set up." He was then led to try oxygen, and in several experiments was successful in restoring the action of the ventricles after the entire cessation of the heart's action.

It would be tedious, and needlessly taking up valuable time, to give more than an epitome of such facts. To comment upon them, *in extenso*, would at present be equally injudicious. I simply wish to intimate that physiology certainly does not negative oxygen in a therapeutic point of view. With a few remarks, as brief as possible, I will pass on to the purely practical portion of this paper.

When we reflect on the acknowledged difficulties and trouble, and prabably many fallacies, attendant upon tedious and lengthened experiments; when these are associated with at least uncertainty regarding facts based on chemical researches; when our still imperfect knowledge of animal chemistry in connection with the vital dynamics and the generation of nervous force is taken into due account; when the fact stands forth that we can, if we please, by means of pure oxygen, induce a state of hyperarterialisation and hyperinosis; when, further, Mr. Erichsen's experiments are brought into the field of mental vision; it must be acknowledged, that the *dicta* of even the most accomplished chemico physiologists as to the uselessness of oxygen in medical practice, ought to be received with extreme caution. Then, add to this the recent views put forth by Professor Schönbein, which a few weeks ago were made the subject of an interesting lecture by Dr. Faraday, at the Royal Institution, and the therapeutic question becomes of still deeper import. It may now be fairly inferred that oxygen can exist in not fewer than three allotropic conditions—ozonic, antozonic, and neutral; the two former even possessing the power of assuming opposite polarities with regard to each other. This interesting discovery is highly suggestive, and opens out a new field for observation and investigation, which, it is earnestly to be hoped, may assist to elucidate this at present obscure inquiry as to the cause of the frequently unquestionable potency of oxygen, when employed in suitable cases *in very small quantity beyond atmospheric proportion.*

Without risking shipwreck upon an obvious yet somewhat vague hypothesis, nearly allied to that so energetically proferred by that acutetold physician, Dr. Stevens, but partially illuminated by the discovery of Schönbein, I will here simply trust that sufficient obscurity and doubt exist in the scientific world, on this interesting subject, to afford ample apology for my differing from authority, while on the present occasion I deviate but very slightly from the practical experience of the clinical observer.

Whatever be the true *modus operandi* of small doses of oxygen (irrespective of the purest atmospheric air), when judiciously exhibited, in many intractable forms of disease, abundant evidence can be brought forward to prove beyond moral doubt that, in certain lowered conditions of the vital forces, this gas can not unfrequently exercise an alterative and tonic influence upon the entire animal economy, which no other medicinal agent at present known can exert. And further, waiving the therapeutic question instances, will from time to time present themselves, during extended employment of oxygen in practice, where certain unexpected, curious, and sometimes unpleasant and serious effects, will so immediately and decidedly manifest themselves as to render it impossible for even the most incredulous observer to impute, *post hoc ergo propter hoc,* expectancy, etc., without ignoring truth altogether.

One mode of casual and limited investigation, for the purpose of testing its action on the system, has often struck me as a fruitful source of error; viz., impulsive trials made by my professional brethren upon themselves and upon their friends, *in a state of health,* or at least where there is absence of any actual disease requiring its use, or of that susceptible condition occasionally met with, through disease or natural temperament, in which a *nidus,* as it were, is presented for the exhibition of abnormal sensitiveness to the action of the gas. Now, oxygen seldom evidences its special influence to any marked extent in persons enjoying perfect health, although rare exceptions are met with; and perhaps it may not be amiss here to add *en passant,* that this fact ought not to be lost sight of, when considering the therapeutic relations of oxygen in derangements of the animal frame. Be it understood, also, that my advocacy of oxygen is exclusively confined to disease otherwise incurable, imminently dangerous, or very intractable.

In employing oxygen in different diseased conditions, it appears necessary to regard its action from two aspects :—1. The alterative and tonic influence which it can exert on the nervous system (apparently irrespective of immediate chemical action *per se*, in the ordinary acceptation of the term), when exhibited in very small doses, and for a very limited period daily. 2. The augmented activity of the normal chemical changes in the animal organism directly induced by bringing a largely increased proportion of the gas in contact with the pulmonary cells; the duration and frequency of the inhalation being, in this case, an important consideration.

So far as relates to the first suggestion, I have, as before stated, had ample opportunities of observing that oxygen can in many susceptible temperaments, and in certain diseased conditions, exercise a peculiarly powerful influence, rarely met with in persons enjoying perfect health; and, by taking advantage of this peculiarity, many cases will quickly undergo a change for the better, where the most enlightened and judicious treatment had previously failed to produce any beneficial effect. In such instances, the gas ought to be used with great caution; for I have known serious and unpleasant (not to sad alarming) symptoms arise from what would usually be regarded as a ridiculously small percentage added to atmospheric air. Most of these examples have occurred in sensitive nervous systems—in individuals possessing a very susceptible nervous organisation; or otherwise in those affected with lesions of the brain or spine, from disease or injuries. Fanciful imagination, hysteria, *et hoc genus omne*, may very naturally create misgivings in the minds of those who have not pernonally witnessed such phenomena. Willingly do we grant a liberal discount to the incredulous, while we add that several of the best exemplifications have presented themselves in men of powerful frames, ignorant of the increase or diminution of the doses that they were being subjected to, and not only devoid of all apprehension of the treatment, but in the highest degree surprised that "a little more pure air" could exert such a perceptible action on the system. The principal symptoms of a disagreeable character, here referred to as occasionally resulting from extremely small doses, are, a sense of constriction of forehead and temples; a feeling of weight over the centre of the parietal bones, and in the occiput; a rush of blood to the head; fulness, pain, or oppressive sensation, in the nape of the neck and base of the brain; sudden faintness, palpitation of the heart; spasmodic contraction of affected parts, *e.g.*, violent reflex movements in extremities affected with paralysis of voluntary motion. Moreover, I have seen, on two or three occasions, a state of unnatural excitement of the entire nervous and vascular systems, which has continued for several successive days after one moderate dose. The chief symptoms of a disturbing character observed from pushing very large doses of the gas, are, in thin anæmic persons, sudden or gradual disappearance of pulse, pallor of countenance, coldness, and partial collapse; in the plethoric and sanguineous, the reverse—viz., too excited circulation; full bounding pulse; intense heat of head, face and skin; severe, oppressive headache. I have also known the frequent and long continued exhibition of it, when not duly superintended, cause much emaciation.

For individuals, however, to be obnoxious to the extremes of the foregoing symptoms, is only occasional.

On the other hand, certain beneficial effects of oxygen may be mentioned as not unfrequently immediate and well marked, where due judgment has been exercised in selecting cases, and in directing the the doses and duration of the inhalation. Such are, complete relief from excessive oppression of the brain; sight improved in defective vision consequent on venus congestion; genial warmth, even to the ends of the toes and fingers succeeding to extreme chilliness and collapsed condition: sudden departure of great nervous depression; permanent relief afforded to the uterus, ovaries, and spine, by sudden induction of long suppressed catamenia, particularly at the change of life; unexpected diarrhœa, of highly offensive character, with dark inspissated bile, in long continued torpor of the liver and portal system; cutaneous transpiration suddenly and freely produced. To guard against any possible misapprehension from dwelling upon these special and important points, it must be distinctly understood that, in very many cases, the beneficial alteration effected by oxygen takes place with no characteristic signs of action perceptible to the patient, or even to the practitioner, except general improvement in the constitution, such as may be observed in many debilitated frames undergoing a mild course of chalybeates. Speaking generally, there will be found, in suitable subjects for the treatment, improved appetite and powers of digestion and assimilation, a feeling of being much more "up to the mark," less lassitude, more ability to bear physical exertion, and (that to which ladies are preeminently partial, and some of the ruder sex not less so) a clearer, fairer, and softer skin.

The semeiological and pathological indications for the employment of oxygen, with a view to cure or palliation, must next receive some general consideration; and afterwards will follow certain points contraindicating its use.

The deviations from health in which it will be found most beneficial are those where there has been no very considerable reduction of what may be called intrinsic vital power. Depression or oppression may be extreme; the nervous and vascular systems may be incapable of receiving more than temporary tone or stimulation by means of ordinary tonics, stimulants, and attention to the best sanitary and dietetic rules; but, as a rule, there must not be present that permanently lowered condition induced by long continued, insidiously undermining nervous nebility (so well known to us all), which, having become almost a second nature, has incapacitated the system for "making life" beyond such amount as is absolutely essential for the maintenance of Psyche in her terrestrial abode.

In some constitutions, with blue noses, conjected conjunctivæ and scleroticæ, semi-stertorous breathing on the slightest exertion, hemorrhoids, &c.—all vividly depicting to the mind the internal state of the matters—each dose will often cause a slow, laboured, full, but very compressible pulse, to become quicker, firmer, smaller. In others, with a small, weak, quick, and even irregular pulse (in the absence of much excitement with hyperæsthesia), the quick conversion into a slower, fuller, and firmer pulsation, is sometimes equally well marked. Examples of each (modified, of course, as to rapidity of production and duration of effect) have frequently occurred in my practice.

The diseases *par excellence* in which the gas has afforded me the most gratification are those attended with either local or general venous congestion,—a preponderance of the venous over the arterial, and torpidity of the capillary circulation. The good effects have been, as a rule, most decided in persons of a gouty or strumous habit, or otherwise in a state of general *malaise*, with sluggish circulation, either constitutional or superinduced by an atonic and oppressed condition through over-feeding and other luxurious or indolent habits, so prevalent in these artificial days.

The organs, which, specially affected, experience the most immediate and sensible advantage from oxygen, are the brain, lungs, liver, and spleen, including the entire portal and mesenteric system, and uterus and ovaries.

In plethoric habits, with chronic local or general congestion interfering with the functions of one or more of these organs, the commencement of the curative process has sometimes been ushered in by sudden and unexpected efforts of nature to throw out peccant matters—efforts for so long a period previously unattainable through the most judicious treatment, and so immediately following two or three large doses of the gas, as to afford almost unquestionable evidence of true sequence. The assistance urgently demanded by the system has been given ; Nature has thus had a starting-point, and a critical discharge has made its appearance. It has been my fortune to see this exemplified in cases of long-suppressed catamenia, in torpidity of hepatic functions with pent-up biliary secretion (as evidenced by sudden diarrhœa of most offensive character), and in gouty affections with much cerebral, nephritic, and other distress. In the last, I have known the urine, which had been for many weeks uniformly clear and limpid, become, to the horror of the patient, turbid, dense, and loaded with urates and phosphates.

I would venture particularly to draw attention to the ascendency of oxygen over the cutaneous capillaries, not unfrequently evidenced from the commencement of its use in torpid and unhealthy conditions of the cutaneous function, and cachexia arising therefrom. The benefit afforded will occasionally demonstrate itself in profuse perspiration, when a dry, harsh skin had been previously the order of the day ; or a relaxed and moist state of the cutis, with constant chilliness and liability to colds, will give place to a warm, healthy, and comfortable state. to which the invalid had been long a stranger. With reference to extremes which occasionally present themselves, it may be added, although any prompt improvement or disappearance of certain eruptions, previously almost untouched by the treatment, will usually evidence the *sauviter in modo* as well as the *fortiter in re* operation of oxygen; yet I have met with a few instances where ladies have been disagreeably surprised to find some slight but chronic eruption become very unsightly after a few days exhibition of the gas. To calm the female mind is not very easy under such circumstances; but a bold assurance of the exacerbation being merely temporary—an effort of Nature to endeavour to rid the system altogether of the disease—has thus far not been falsified by the result.

Even inveterate skin-affections, the history of which points to a congenital origin, and incurable in the permanent sense of the word, may nevertheless receive much benefit from an occasional resort to this remedial agency.

Rapidly spreading ulceration, with sloughing nearly allied to gangrene, has under my observation been suspended solely through the influence of oxygen in a few liberal doses. A similar result has followed its employment in malignant anthrax, the most serious symptoms being brought under permanent subordination. And, indeed, I firmly believe, with oxygen as an adjunct, surgeons might regard with much less dread the frequently alarming and even fatal supervention of erysipelas or gangrene in traumatic cases. Conjoined with proper sanitory measures and the usual treatment, abundant stimuli, beef-tea, cinchona, ammonia, &c., the inhalation of the gas, might, in some cases of the worst description, prove the salvation of the patient. Perhaps this suggestion might not inaptly be extended to the notoriously fatal effects of badly located hospitals.

The space to which I must necessarily limit myself in this paper, will admit of little more than a glance at those pulmonary and cardiac affections, in which oxygen merits cautious, careful, and judicious trial. I will simply remark, that the palliative effects of oxygen have kept hopelessly phthisical subjects in comparative comfort and freedom from dyspnœa and exhausting cough, up to their dying hour; while the necessity for opium and other sedatives has been very trifling, and comfortable sleep has been secured, *minus* any disagreeable feelings following the free use of the medicines. I know a lady in London who, with fatty degeneration, and probably attenuation of the muscular walls, and enlargement of the cavity of the heart, has been kept alive for the last three years by a dose of oxygen almost daily. On many occasions, in this lady's case, the heart's action, capable of only partial and temporary restoration by brandy and other stimulants, has at once been restored by oxygen in large doses. She lives by exercising great care and avoiding physical exertion, to a certain extent enjoys life, yet cannot do for more than a few days successively without the renovation afforded by the gas. Again, in asthma, there is a tolerable percentage of cases in which oxygen can either cure or greatly relieve. As a rule, the subjects most benefited are those in which we can trace no congenital or hereditary predisposition, and where the disease owes much of its origin to chronic bronchitis or partial congestions and indurations of the pulmonary parenchyma.—*British Med. Journal, Dec.* 24, 1859, *p.* 1033.

144.—ARTIFICIAL ANÆSTHESIA.

[M. Azam, of Bordeaux, has lately published the result of some experiments which appear at present to be exciting much interest in Paris. The foundation of these is a work published in this country by Mr. Braid, in 1842. This gentleman's views on the subject were embodied in a paper in the Retrospect, but we think that it it will refresh the memories of our readers, to give the subjoined remarks.]

The patient, either sitting up or lying down, is put in a convenient position. The operator then, standing either before or behind him, places before his eyes, at the distance of a few inches, but generally nearer than the point which allows of distinct vision, some bright object, upon which the patient should steadily and continuously fix his eyes. The bright object should be so placed that the eyes, in looking at it, must be forcibly directed upwards, the contraction of the superior recti being carried to its maximum degree. In this position, the levatores palpebrarum and recti are strongly contracted, and convergent strabismus takes place. After this attitude, which is certainly fatiguing, has been kept up for two or three minutes, the pupils are noticed to contract, and soon afterwards to dilate; the eyelids quiver rapidly, then fall, and the patient is asleep. Two symptoms, almost always, present, are then observed; they are, however, in different cases, more or less marked and lasting: 1, catalepsy, exactly as described in books; 2, anæsthesia, which lasts from three to fifteen minutes, either complete or incomplete, but which allows of pinching, pricking, and tickling, without any feeling being aroused in the patient, and without any change in the cataleptic state being produced. This anæsthetic state is generally followed by a very opposite condition—namely, very remarkable hyperæsthesia, in which the senses, the feeling of heat, and muscular activity reach an unusual degree of excitability. At any moment of the experiment the symptoms may suddenly be stopped, by rubbing the eyelids, and directing upon them a stream of cold air. When the patients recover their senses, they remember nothing of what has taken place.

Several experiments have been instituted in Paris by Messrs. Follin, Broca, and others; and M. Velpeau seems so convinced, that he has presented a short paper on the subject, by M. Broca, to the Academy of Sciences at the meeting of the 5th inst.

The *Gazette Hebdomadaire* of the 9th of December mentions the following case:—A woman aged, twenty-four, rather nervous and timid, had, in consequence of a burn, a large abscess by the verge of the anus, and was told that she would be narcotized before it was opened. A bright brass tube (a telescope made by Bruecke) was placed five inches in front of the nose. The patient was obliged to squint considerably in order to look steadily at the object, the pupils contracting very strongly. The pulse, which before the experiment was quick, became now quicker, but immediately afterwards weaker and slower. After a couple of minutes the pupils began to dilate, and the left arm being artificially lifted up vertically above the head, remained motionless in that attitude. Towards the fourth minute the answers became slower and almost painful, but perfectly sensible, and the respiration slightly irregular. At the end of five minutes, M. Follin pricked the skin of the left arm, which was still held up at a right angle with the trunk, but the patient did not move. Soon afterwards a puncture was mode, which drew a little blood, but no feeling was evinced. The right arm was now placed in the same attitude as the left, and the region where the abscess was situated brought into view. The patient yielded willingly, saying, very quietly, that she was doubtless going to be hurt.

Finally, about seven minutes after the beginning of the experiment, M. Follin laid the abscess largely open, and freed a great quantity of fœtid pus. A faint cry, which lasted less than a second, was the only sign of reaction which the patient gave. No movement of the muscles of the face or the limbs was observed; and the arms remained in the same cataleptic state which they had previously assumed. Two minutes later, the attitude was still the same: the eyes wide open and a little vascular; the face motionless; the pulse as it was before the experiment began; the breathing quite free; and the patient insensible. The left heel was now raised, and it remained unsupported in the air, whilst the cataleptic state of the arms persisted. M. Broca at this period removed the bright object which had hithertoo been constantly kept before the patient's eyes, gently rubbed her eyelids, and directed upon them a current of cold air. She now made a few movements, and was asked if she had felt anything; upon which she answered that she did not know. Both arms and the leg remained, however, in the artificial position in which they had been put. At this stage the left arm was again pricked, and no sensation thereby excited.

Eighteen minutes after the beginning of the experiment, and twelve after the operation, another friction on the eyelids and another current of cold air was used; whereupon the patient awoke almost suddenly, the cataleptic limbs all falling together. The patient then rubbed her eyes, came to her senses, remembered nothing that had passed, and was surprised that the operation was over. Her state was somewhat analogous to that of patients who wake from anæsthesia induced by ordinary means; though the waking was certainly more sudden, and without agitation or talking. The anæsthesia. which had thus been artificially interrupted, had lasted from twelve to fifteen minutes.

Two attempts of the same kind have been made by MM. Azam and Follin, in the same hospital, upon a girl aged eighteen, who was affected with a slight wound of the foot; but the results have not been so satisfactory as they were in the last case. Two other experiments which were undertaken by M. Azam on the 8th instant were more successful. In a young woman, catalepsy began in a minute and a half, and in two or three minutes both catalepsy and anæsthesia were complete. With another woman, suffering from cholera, anæsthesia was well established in less than two minutes. A third experiment was tried in the presence of M. Trousseau, upon a girl who had been for some time in the hospital for epileptic vertigo. In a minute and a half, by means of a pair of scissors held ten inches from the eyes, she was cataleptic and asleep; and when awakened, she complained of lumbago and much fatigue; altogether she remained in a state of hebetude and stupor much longer than happens after recovery from epileptic attacks.—*Lancet, Dec.* 17, 1859, *p.* 620.

145.—POISONING BY CARBONIC ACID GAS, WITH RECOVERY.

By J. B. Thompson, Esq., L.R.C.S., Edin., Resident Surgeon, General Prison, Perth.

[The subject of this report, a man of twenty years of age, was employed in his prison-cell, picking oakum, when an accidental spark igniting the teazed material, the cell was immediately filled with smoke. After ineffectual attempts to extinguish the flame, he fell down stupified. An official hearing a cry made by the man opened the door within a minute and a half, when "the prisoner walked out of the cell without help."]

The prisoner assisted to put out the flames, which was done in a few minutes. He spoke to the officer, and explained how the fire originated. He walked about the length of 150 yards, carrying water in a pail. In about five minutes from the time of issuing from his cell, the prisoner sat down upon a stool, became sick and stupid, and fell on the floor in a state of insensibility. He was raised up and replaced upon the stool, had a drink of water, and his face and hands washed; and a second time he fell on the floor, was seized with retching, foaming at the mouth, spasms, and total insensibility.

The following is the description of the patient's condition from my note-book at the time:—

Symptoms—Nov. 9th, 5.50 p.m.—Within twenty-five minutes after the accident of the fire I saw A. S. He was lying prostrate, rolling his body from side to side, tossing about his arms, convulsed as if by tetanus; when his body was set up, he showed an utter want of muscular power. The face was pallid, eyes shut, pupils fixed and contracted. His head, when lifted up, was drooping and fell down; retching constant, vomiting of frothy saliva in large quantities; pulse full, languid, numbering 68; total insensibility. The vomitings were darkish, and contained carbonaceous matter. When raised to the upright posture he had no locomotive power, and when pinched no sensation of pain.

Treatment.—I had him exposed freely to a current of fresh air, made him inhale ammonia, and bled him from the arm to the extent of ℥x. Cold affusion was applied to the surface of the body.

The blood appeared somewhat darker than normal.

The symptoms continued as described until past midnight without amelioration: during which interval he had leeches viii. applied to the temples and a purgative enema; cold applications to head. 12 *midnight.*—The retching and vomiting of frothy saliva, with stupor, unabated.

November 10th. 7.20 am.—Pulse 70, full and labouring; respiration oppressed, lays his hand on his breast as if to express his labour in breathing; respiratory murmur dull; cannot speak, cannot swallow anything; comatose. The vomiting of frothy matter has partially subsided since midnight. A blister was applied to the chest.

This condition remained with some slight improvement throughout the day. The frothing at the mouth recurred for two or three hours occasionally; the stupor seemed less at times, then returned as profound as ever. Still some carbonaceous matter in the vomitings. On examining the nostrils and fauces, I found them much inflamed; at times made signs by opening his lips, but spoke none. Tried to swallow, but with evident pain and sense of choking; and the irritability of the stomach continued such, that if anything was swallowed it was at once rejected.

Nov. 11th and 12th. With slow but steady amendment, all the symptoms became more favourable; cannot remember what occurred at the fire.

Nov. 13th. He was able to take and retain nourishment; having been previously kept up by nutritious enemata. He now speaks a word or two and sinks down into a partial stupor. Complains of great pain in the nasal passages and the fauces, which are still slightly inflamed.

Nov. 14th. He is now so far well as to sit up a little; much reduced in strength, and, as it appears to me, his mind, never strong, rather enfeebled. Does not remember assisting to put out fire in cell.

Some of the features of this case seem very remarkable:—

1. *The interval from the time of exposure to the carbonic acid gas and its effects* was considerable. I have taken some pains to make out, as near as possible, the length of time that elapsed before the

stupor and sickness appeared, and there must have been *at least five minutes.* During this interval, was the poison working its way into the system? In assisting to put out the flames he may have imbibed some more smoke, but this cannot have been of any consequence.

Among the cases of poisoning from this gas, we find that the miners who worked in the burning mine belonging to the Devon Company sometimes wrought for a length of time before they were affected; and the only test of the poison being concentrated and dangerous was the effects produced. The air was pure enough to support light, but yet injurious to life, by its gradual and cumulative effect upon the system. In the black lung of miners I am satisfied that carbonaceous matter, in many instances, is thus *ab extra* carried into the circulation; and, from its extreme solubility in the blood, there is no difficulty in believing this theory. M. Nysten found that 64 cubic inches of this gas may be absorbed without proving injurious. It thus appears that the effect of carbonic acid gas is not always sudden and immediate; but by repeated and cumulative doses, it may, after a longer or shorter period of exposure to its influence, be absorbed into the system, and act as a positive poison.

2. The effects were decidedly narcotic. The symptoms of stupor, laborious circulation and respiration, with convulsion, all indicated deep narcotism. For several days after complete recovery, the patient remembered nothing of what occurred during the interval from leaving his cell until he became insensible.

3. The excessive frothing from the mouth was the most rare and striking feature in this case, and seems to be characteristic of poisoning by various of the gases. The amount of saliva discharged was beyond anything I have seen in the same space of time from the most severe salivation.

The history of this case seems to leave no doubt that carbonic acid gas was gradually absorbed into the system, and acted as a *positive poison.*

That gases may permeate freely through the human body cannot be doubted. We see this in cases of injury to the lungs when emphysema occurs and inflates all the tissues, the air again passing out by the lungs and skin. It is also well known that a stream of carbonic acid gas made to play upon the membrane of a frog's foot, stagnates the capillary circulation there. Is this a chemical or a mechanical effect? Is this asphyxia proper, or is it the action of a positive poison on the blood? The latter opinion is all but proved.

Frequent and fatal accidents occur from the feather-white wine (*der federweisse Wein*), which from the heat disengaged in the stomach, evolves carbonic acid gas in large quantities by fermentation, and penetrating through the stomach, diaphragm, and other membranes, reaches the blood, and produces the very symptoms which occurred in the case before us, and often ends fatally.

All such cases are explained more readily than by any other theory, when we assume the doctrine that carbonic acid gas acts as a narcotic poison.—*Edin. Med. Journal, Jan.* 1860, *p.* 642.

146.—ON IDIOSYNCRASIES.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

All the phenomena which take place in the living body, whether in health or in disease, are, in almost every case, capable of being rationally interpreted, and distinctly traced back to the action of those laws which regulate the vital functions, both in the normal state, and in its various deviations.

We learn, however, from daily experience, that whatever their general effects may be, the causes of disease are far from acting with equal intensity upon the various individuals who are exposed to their influence. Cold, hunger, thirst, fatigue, and moral suffering;—are not these the constantly recurring causes of sickness? and are they not, in some measure, the lot of the whole species? How, then, does it occur, that among those who daily undergo their action, certain individuals are found to give way, while others resist? and, when epidemics are raging in given localities, how does it occur that some persons only are affected with the prevailing distemper, while others who live in constant communication with the victims escape unharmed? To the mysterious power which thus modifies, in each particular case, the influence of external agents, we give the name of Idiosyncrasy.

We may, I believe, take it for granted, that not only morbid, but also physiological predispositions exist in man as well as in the lower animals; even in a perfect state of health each individual retains his own peculiar habit of body, and is, in consequence, more liable to certain accidents than his neighbour. The various animals which serve for our experiments are far from exhibiting the same phenomena, under the influence of agents entirely similar in their nature. You are already aware that as we rise or descend in the scale of being, we find animals endowed with different degrees of sensibility to the action of certain poisons—those, for instance, which operate more especially on the nervous system. There exist, therefore, within the limits of health considerable differences between living beings; and, as we have previously established, these various properties are not merely the result of organization, but frequently depend on the condition in which the animal has been placed. In this manner, a rabbit may be brought down to the level of a bactrachian; and, by reversing the experiment, the inverse result may be obtained. Now, these important modifications are almost invariably produced through the agency of the nervous system.

Not only do the various species of animals differ in this respect; but even individuals belonging to the *same* species are so far from resembling each other, that they cannot be submitted to the same experiments. So exquisite is the nervous sensibility of dogs of the higher breed, that the slighest operations bring on fever, and are attended with alarming symptoms; they cannot, therefore, be employed in researches connected with the gastric juice, the pancreatic secretion, etc.; in fact, all operations performed within the abdominal cavity are liable to superinduce peritonitis in those highly sensitive animals, and generally prove fatal. In dogs of a more vulgar class, how different are the results of similar experiments? During the operation, the animal hardly attempts to move, and scarcely seems to suffer; the appetite remains unimpaired, and the secretions normal; in short, the various functions of the economy pursue their natural course.

In the horse these differences are, if possible, still more strongly marked. The characteristics of certain breeds are, in colloquial language, attributed to *blood;* it would be more correct to attribute them to *nerves;* an irritable, sensitive, and highly organised nervous system is, in fact, the essential difference which separates a race-horse from one of those diminutive half-wild ponies which hilly countries so abundantly produce. Would not the results of the same experiment be entirely at variance in these different animals? and what comparison could we possibly establish between them? It is, therefore, indispensable whenever great powers of endurance are required for the purposes of scientific research, to select an animal of the lower breed; if, on the contrary, sensitiveness and nervous irritability appear desirable, none but the nobler kinds will afford the requisite qualities. Experiments on recurrent sensibility, for instance, which, in the greyhound and pointer, are generally successful, if tried on a shepherd's dog would fail in almost every case. Cold-blooded animals stand, of course, in this respect, at the very bottom of the scale. It will, therefore, easily be conceived, that a state which in certain animals would constitute actual disease, may be perfectly natural to others.

The difference between individuals may be naturally expected to be far more extensive in man, than in all other living beings; and, if we might venture to allude to a subject which actually occupies the public attention, is not hypnotism a peculiar state, which can only be superinduced in a small number of highly sensitive and nervous patients? and do not all the phenomena of mesmerism, somnambulism, and similar nervous symptoms, fall under the same general rule? It is, therefore, evident that idiosyncracies are only peculiar susceptibilities which exist, in the normal state, in various individuals.

Up to this moment, however, we have only examined physiological and innate predispositions, so to speak; but, as Medical men, we are far more deeply interested in the investigation of accidental, transitory, and morbid idiosyncracies. To ascertain the circumstances which may be supposed to give them birth, is, to the physiologist, a most important object of research.

If we compare an animal in a state of abstinence to one in full digestion, the most evident discrepancies will be noticed in the results of experiments simultaneously performed upon them. A dose of strychnia which almost immediately kills the second, will not act before a certain lapse of time upon the first. The powers of absorption have, of course, been called into account for so remarkable a fact, but we are aware that absorption, in a state of abstinence, is more active by far than during the process of digestion ; the explanation is therefore unsatisfactory. The lowering of the physiological activity of the nervous system is, in reality, the only cause to which the difference can possibly be referred. When deprived of food, the animal gradually sinks in the scale, and acquires properties altogether foreign to its previous state. Is this a morbid condition ? No ; but the natural result of a well-known physiological process.

We therefore entirely deny the existence of a so-called Morbid Physiology, if by this expression a state of things entirely independent of the ordinary laws of life, is to be understood. Such expressions ought to be expunged from the book of science ; they only serve to render our notions confused, and lead the student astray. When speaking of Medical Chemistry, for instance, we do not pretend to say that the chemical actions which take place within the living body are totally distinct from those observed without. Morbid physiology has, no doubt, its laws; but they are precisely the same as those which regulate, in the healthy state, the vital functions.

Not only abstinence, but cold and various other causes, modify the conditions of life, and alter the results of our vivisections ; under a low temperature, cold-blooded animals grow less sensible to the action of certain poisons ; a larger dose of strychnia is required to kill a frog in winter than in summer. But chloroform, ether, and even mere ordinary inebriation produce similar results ; and, in America, it appears to be generally understood that intoxication is a preservative against the bite of the rattlesnake.

These, however, are the physiological modifications of the system. Our purpose is to investigate those which pertain to the morbid state. It is a well-ascertained fact that medicines do not act on sick people in the same manner as on persons in the full enjoyment of health. Now, the biological conditions superinduced by disease evidently lie at the root of these irregularities. To adduce a well-known instance of this, wine, brandy, and ardent spirits—so freely used by certain American physicians in the treatment of low fevers, remain apparently without effect on the patient, even when administered in quantities which, in a state of health, would inevitably produce intoxication. A two-fold explanation of the fact presents itself—Firstly, the process of absorption is almost entirely suspended; Secondly, the nervous system is strongly depressed. You are, of course, aware that, in cases of typhoid fever, the absorbent powers lie dormant for a long space of time; a fact established by the following experiment—If small quantities of prussiate of potash are dissolved in the patient's drinks, no vestige of of this substance is discovered in the urine, or in any other secretion. A similar state of things may be physiologically superinduced; for where secretion is over-excited absorbent surfaces lose their properties. The inner surface of the salivary gland, which in a state of rest, rapidly absorbs strychnia or woorara, ceases in some measure to do so when secretion is going on. Five cubic centimetres (one-third of a cubic inch) of an aqueous solution, containing one-hundredth part of strychnia, being injected into the parotid duct in a dog, the animal was almost instantaneously killed. The same experiment being tried on another, in which secretion was kept up by means of galvanism, life was protracted for the space of twelve minutes.

Cholera is evidently another instance of the same fact. No substance whatever is absorbed by the intestinal walls, as long as this abundant and characteristic serous discharge continues. But disease, it will be said, is the origin of all these modifications. True; but a physiological process takes place under its influence, and the facts observed are its natural results.

The deficiency of the absorbent power has equally been found to exist under circumstances entirely different from those we have just examined. It occurs, for instance, in mania. Nervous influence here appears to be the sole agent; for, as soon as the acute crisis is past, the process of absorption recommences, as in the healthy state.

Woorara has been used of late in the treatment of tetanus. Out of four cases two have recovered—the other two died; but, in both the successful cases, the ordinary—or, so to speak, the physiological—effects of the poison had been produced; while, in the other two patients, no such resultw as obtained. Doubtless some peculiar condition of the nervous system, rendering all impregnation impossible, existed in these two latter cases; perhaps, if treated at an earlier period of the disease, the patients might have recovered. In this respect, the well-known effects of sulphate of quinine may be adduced. When administered in large doses (*e. g.* in cases of acute rheumatism), it never abates the pulse nor relieves the other symptoms without previously occasioning deafness—a physiological effect intimately cennected with its therapeutical agency, for when, after improving the patient's condition, the dose is too abruptly diminished, the deafness disappears, and all the symptoms previously kept down break out again in full force.

But we also find in animals various predispositions, which not only modify the action of medicines administered to them, but also render them liable to diseases entirely different, when suffering from causes entirely similar. Being about to perform certain experiments on animals kept fasting for a long space of time, I left some dogs without food for several days; but, during the late severe frosts, these animals died unexpectedly. In making the autopsy, we discovered pneumonia in one case, pleuritis in another, and inflammation of the bowels in the two last. Thus, under conditions perfectly identical, these animals were affected with totally different diseases. But similar results may be obtained at will by the physiologist. When rabbits are placed under total abstinence they generally live a fortnight or three weeks; but when certain branches of the sympathetic nerve have been previously divided, the animals die within a few days when deprived of food, through acute inflammation of the viscera connected with the nervous twigs that have been divided. When, some time ago, I commenced this series of experiments, I discovered that the section of large divisions of the sympathetic nerve was apparently unattended with the slightest inconvenience as long as the health of these animals remained perfect. Some of them even became pregnant and brought forth their young; but, as soon as a general debilitation of the system arose from want of proper nourishment, acute inflammation was produced in the organs deprived of nervous influence. We had, therefore, succeeded in artificially creating particular idiosyncrasies in these animals, and could predict with perfect certainty that as soon as health failed disease would arise on a given point.

Morbid predispositions must, therefore, be viewed in the light of peculiar physiological conditions, which, in most cases, depend upon the nervous system; and an immense progress would be realised in medicine, if it were possible to diagnosticate in a state of health the predisposition to disease, and foretell the coming danger. A Russian army physician, who had invented a new sphygmometer, and had applied it to the study of various diseases, stated some years ago, that during a severe epidemic of cholera, a peculiar slowness of the pulse existed, several days before the explosion of the disease, in those who were marked out to be its victims. I am not aware whether the reality of the fact has been ascertained by other observers; but it would be, at all events, a most precious boon to know beforehand when epidemics prevail, what persons are more particularly liable to be affected with the reigning distemper; we should thus be far better able to adopt preventive measures, and prescribe hygienic regulations.

In conclusion, let me advise you not to consider idiosyncrasies in the light of mysterious powers residing within the depths of our organs, nor as entirely novel functions superadded as it were to those which already exist; they must be viewed as mere natural manifestations of the ordinary laws of physiology.—*Med. Times and Gazette, Feb.* 4, 1860, *p.* 109.

147.—*To make a Blister.*—Steep cantharides in æth. sulph. for a fortnight, or until the cantharides floats upon the surface; skim it off. One drachm of cantharides, ditto of white wax, five ditto of olive oil, melted together, mix. With a brush paint it over some white bibulous paper, and hang it up to dry in a current of air. Take a piece of pink paper, form and size required, and paint the undercoloured side over with a weak solution of India rubber; cut your cantharide paper the form and size (less a margin) of the pink paper,—while the India rubber solution is still sticky place it on, when dry roll it up. It is unaffected by damp, is light, portable, blisters with certainty and without pain. The introduction of the caoutchouc varnish arrests the perspiration of the part and increases doubly the certainty while diminishing the time required for application. Before applying, the blister should be held over the steam of hot water. The same blister will be effectual for several times.—*Dublin Hospital Gazette, Jan.* 2 1860, *p.* 15.

148.—ON TOXICOLOGY.

By Edmund W. Davy, M.B., M,R.I.A., &c,.

In Toxicology much advance has been recently made. The cases of Palmer, Dove, and others, have been the means of directing public attention to that fearful and subtle poison, strychnine, of which but little was known prior to those criminal cases; and a great amount of knowledge has been gained as to the chemical and physiological properties of that most active poison. Many new and most delicate tests have been discovered which are found capable of detecting with certainty the most minute quantities of strychnine. I shall only refer to a few of the most characteristic.

One class of tests depend on the colour produced by the action of different reagents on strychnine, and are therefore called the colour tests; of these by far the most striking and delicate are bichromate of potash and the ferrocyanide of potassium, which in conjunction with strong sulphuric acid produce with strychnine a most beautiful purple coloration. The first was discovered by M. Lefort, and the latter by myself a short time ago; and they appear to be the most delicate tests of strychnine yet discovered. According to the experiments of Professors De Vry and Der Burg, of Rotterdam, on the comparative delicacy of different tests for strychnine, they state that those are the most sensitive, and that they were able by their means to detect the one-sixty-thousandth part of a grain of that alkaloid.

When search is made for the presence of strychnine in the contents of the stomach or in the different tissues or organs of the body, we are obliged to isolate or separate the strychnine from the various organic substances with which it must necessarily be mixed, in such cases, previous to the application of these and other tests; and many different processes have been adopted for that purpose, the details of which would be out of place on the present occasion; but I may observe that very recently chloroform has been used to separate strychnine from organic matter, and it appears to be a very efficient substance for this purpose.

Professor J. E. Rodgers and Dr. Girdwood, of London, succeeded by the use of chloroform in detecting strychnine in the blood, liver, stomach, and tissues of animals which had been poisoned by that substance; and in one case after the animal had been interred for a year; which latter is a most important fact, as it was an opinion very generally entertained that strychnine underwent decomposition when it was taken up in the system, which was the reason why it could not be detected in the blood and different tissues after death in several cases of undoubted poisoning with that alkaloid. From the experiments instituted by Professors De Vry and Der Burg, they are led to the following conclusions, which seem to be the most consistent with known facts: viz., that if death has been caused by strychnine, this poison can only be detected in the body provided it has been administered in a quantity more than sufficient to cause death ; but that if the poisoning by strychnine has been chronic, and has resulted from a quantity not greater than just necessary to cause death, it cannot be detected by a chemical examination of the stomach and intestines; and that it is probable that that part of the strychnine which has destroyed life is decomposed in the living body.

The only other test I shall call your attention to is the beautiful test of Dr. Marshall Hall, which that distinguished physiologist proposed a short time before his death. It consists in producing the peculiar physiological effects of strychnine in animals.

He selected for this purpose a frog, which he placed in a very dilute solution of strychnine, when after a short time it became tetanic and exhibited many of the effects peculiar to this poison.

This test is one of considerable delicacy, for Dr. Hall found that a frog placed in six drachms of water containing only the one-four-hundredth part of a grain of the acetate of strychnine became, after three hours and a-half violently tetanic, and in two other experiments, the one-five-hundredth, and even the one-thousandth part of a grain of strychnine were thus detected.

Dr. Harley, of University College, London, says, that he considers the physiological test the most reliable one for strychnine; and the method of applying it appears to be by injection into the thoracic or abdominal cavity. He further remarks, that when the poison reaches the lungs it seems to act most speedily; which is easily explained by the rapidity with which the poison will be absorbed into the blood through the pulmonary capillaries. He found that a solution containing so small a quantity as the one-sixteen-thousandth part of a grain of the acetate of strychnine, which is equal to the one-eighteen-thousandth part of a grain of the pure alkaloid, injected into the lungs of a small frog, caused it to become violently tetanic after nine minutes and a-half, and to die in two hours.

Though the physiological test for strychnine is one of very great delicacy, particularly when applied as Dr. Harley recommends, still it is not taken by itself perfectly conclusive, at least in the case of frogs, which are now known to be tetanised by different causes unconnected with the presence of strychnine; there can, however, be no question that this test may often be used as a most valuable adjunct to the chemical tests, and more satisfactory evidence may be thus obtained as to the presence or absence of that poison in suspected cases.

I may observe in connection with this subject, that as a general rule, those substances which have been found to possess the most deleterious effects on man and animals, have by divine foresight been endowed with very marked and characteristic chemical properties, which enable us readily to distinguish and identify the presence of those bodies, as, for example, arsenic, corrosive sublimate, prussic acid, and, as we have just seen, strychnine; and though no doubt there are many vegetable and animal poisons which appear to possess no decided chemical properties, yet there can be little doubt but that when they come to be more attentively studied, they will be found likewise to possess characteristic chemical effects by which they may be readily known. The identification of those substances which are destructive to human life, is of very great importance to society; and the public owe a deep debt of gratitude to chemists and other men of science who have studied and made known the properties of many of those substances.

But to return to strychnine, the inquiries recently made in connexion with this substance, have not only been the means by which different methods have been devised for the detection of the most minute quantities, but, likewise, several antidotes have been proposed by which the poisonous effects of strychnine may be more or less destroyed.

One of the most remarkable substances as yet proposed as an antidote for strychnine is nicotine, the active principle of tobacco, which was suggested by Professor Haughton, F.T.C.D. Mr. Haughton was led to try the effects of nicotine as an antidote from the opposite physiological properties of that substance when compared with those of strychnine (the former producing relaxation, and the latter contraction of the muscles), and he conceived that it was possible, from their opposite effects, that they might mutually neutralize, as it were, each other's action. To determine this important question he instituted several interesting experiments on frogs, which he placed in different solutions of strychnine and of nicotine, and the results clearly show the correctness of his inference as to nicotine being more or less an antidote to strychnine.

These interesting results Mr. Haughton communicated to the Royal Irish Academy in 1856, and he has recently published them in a separate pamphlet, together with an interesting case of attempted suicidal poisoning with strychnine, which occurred in St. Louis, Missouri, United States, where, acting on the suggestion of Mr. Haughton, the individual was successfully treated with an infusion of tobacco.—*Dublin Hospital Gazette, Dec.* 17, 1859, *p.* 371.

149.—ATTITUDE OF PERSONS KILLED BY LIGHTNING.

"That which particularly characterises the effects of lightning," writes M. BOUDIN, "is its suddenness. At one time the individual is killed dead on the spot, remaining in a sitting posture, or even on horseback. At another time he is thrown a long distance." The first mention made of this kind of death is in the history of Alexander: "During a dreadful tempest," says Quintus Curtius, "which destroyed a thousand men, some were found supported against trees, as if still alive and talking with each other, and just in the attitude in which death had surprised them." According to Carden, as quoted by Rivière, eight reapers taking their food under an oak were struck by lightning, and died preserving their attitude—one of a man eating, another drinking. In Lorraine a woman and one of her children were killed, and remained in a sitting posture. At Dover, a man killed with four horses was found sitting under a bush. A man of law at Troyes was killed by lightning when on horseback; the animal still continuing his journey, brought home his dead master, retaining the posture of a man on horseback. On July 11, 1819, at Château-neuf, three fire-balls fell on the church. Nine persons were killed and eighty-two wounded. All the dogs found in the church were killed, and retained the attitude which they previously had. On January 22, 1849, a goat was killed near Clermont, and found sitting upon his hind-quarters with a branch of green leaves in his mouth. Now this phenomenon is sometimes equally observed in persons killed by fire-arms. In the 'Morning Herald' of November 8, 1854, the following details are given of a visit to the field of Inkermann, immediately after the combat: "Many faces still seemed to smile; others had a threatening look; some bodies had a funereal *posé*, as though laid out by friendly hands; others still knelt upon the ground, convulsively grasping their weapon, and biting a cartridge. Many had their arms raised as if endeavouring to ward off a blow, or as if desiring to offer a last prayer. All their faces were pale, and the fierce blowing wind seemed to animate their dead bodies; one would have said that these long lines of the dead were about to rise to recommence the struggle." M. Boudin writes the same thing of the appearance of many of the Russians after the battle of the Alma: "Some seemed still writhing in the agonies of despair and death, but the most wore a look of calm and pious resignation. Some appeared to have words floating on their lips, and a smile as in a sort of high beatitude. One was particularly observed, his knees bent, his hands raised and joined, his head thrown back, murmuring his supreme prayer." At Magenta again many dead bodies, as we are informed by

Surgeon-Major Armand, of the Army of Italy, maintained the attitude they had when struck, passing instantaneously from life to death, without agony or convulsion. A Zouave struck point-blank in the chest, still held his bayonet in the position of the charge, with the menacing aspect of a dead lion. His Majesty the Emperor is said to have remarked a similar case at Palestro. Near to the Zouave was an Austrian, dead from hemorrhage, lying in a pool of blood. His face and eyes were turned to heaven, his hands joined, and fingers stiffly interlaced, evidently in the attitude of prayer.—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 167.

150.—ON THE CEREBRO-SPINAL FLUID.

By Joseph Williams, M.D.

Nearly five-and twenty years have now glided by since my attention was first specially directed to the secretion and probable uses of the cerebro-spinal fluid, by reading in 'The Lancet' an account of the interesting experiments of Magendie. On the following session I attended a course of his lectures at the College Royale de France, and ever since that period my inquiries have been, from time to time, devoted to this abstruse and complicated subject.

Desirous that the views now promulgated may be duly tested, weighed, and sifted, I place them in the pages of 'The Lancet,' in order that other opinions may be elicited as to their validity. Some of the suggestions may at first appear somewhat startling, although upon patient investigation they will, I believe, gradually become more and more feasible.

The following statements, then, some of which we know to be matters of fact, while others are, in our present state of knowledge, *necessarily hypothetical,* I now venture to bring forward.

That the cerebro-spinal fluid occupies that space which exists between the inner layer of the arachnoid and the pia mater, and that, as pointed out by Magendie, the ventricles of the brain communicate with this sub-arachnoid cavity.

That the fluid in the sub-arachnoidal spaces, and points of confluence, freely communicate with each other, and also with the ventricles of the brain; and further, that the cerebro-spinal fluid readily passes from the cranial sac downwards to envelope the spinal cord, and also upwards from the spinal sheath into the cranium.

That the central canal in the spinal cord *invariably exists, and bears the most evident marks of special design in its peculiar construction.*

That the intra- and extra-cranial sub-arachnoidal tubes, surrounding the cerebral nerves as they emerge from the cranium, communicate continuously and directly with the fluid in the large intra-cranial sub-arachnoidal spaces, and, consequently, also, with the spinal sub-arachnoidal sac.

That the filaments of the roots of the cerebral and spinal nerves may be clearly seen floating in the cerebro-spinal fluid.

That the clear fluid which sometimes escapes so abundantly from the ear and from the nose in cranial fractures, is now proved to be cerebro-spinal fluid.

That the quantity of the cerebro-spinal fluid is infinitely greater than is usually imagined or described; and that it is not only rapidly secreted, but also rapidly renewed.

That it is doubtless being constantly secreted, there being a continuous *waste*, not only with each psychical, but with every physical and reflex action; and that a large amount of cerebro-spinal fluid is probably formed for psychical than for mere physical effort.

That it is probably being continuously excreted by cuticular, but especially by renal action.

That the cerebro-spinal fluid becomes at once affected, tainted, impregnated, or poisoned, by alimentary, chemical, or therapeutic agents —by whatever, in fact, enters into the circulation.

That the cerebro-spinal fluid actually comes in contact with what we believe to be the most important portions of the cerebro-spinal mass; *and that it also probably is distributed throughout the whole of the brain and spinal cord, and even to the minutest peripheric terminations of the nerves.*

That the quantity of the cerebro-spinal fluid bears an inverse proportion to the size of the cerebro-spinal axis; and that, in obedience to the hydrostatic law, the fluid instantly passes to the point where the resistance is least; but that intracranial compression may actually occur, notwithstanding those numerous and wonderfully designed safeguards for preventing it.

That the cerebro-spinal fluid is probably secreted by the cellular portions of the cerebro-spinal mass, or by the pia mater, and that it is being constantly diffused throughout the whole of the nervous system.

That the pia mater not only dips down into the convolutions, as usually described, but that it probably, as a highly vascular and delicately attenuated membrane, *penetrates into every portion of the cerebro-spinal mass, wherever the arterial blood is distributed.*

That sea-sickness probably depends upon the irritation caused by the irregular and unequal vibratory pulsations or undulations of the cerebro-spinal fluid, as also the *anti-peristaltic action* so frequently observed in hydrocephalus, as well as the less frequent intussusception.

That the alternate rise and fall of the cerebro-spinal fluid does not synchronize with the pulsations of the heart, but corresponds with the movements of *respiration*, there being an alternate compression and exhaustion, and hence the dynamic power for sustaining the circulation of the cerebro-spinal fluid.

That undulatory motions or currents exist in all the cavities of the brain and spinal cord.

That in addition to the cerebro-spinal fluid forming a "water bed" on which the cerebro-spinal mass can float, and thus be protected from friction and concussion, and in which its nutrient bloodvessels can also be protected from any direct pressure, that it is also probably destined to pervade and moisten every portion of the nervous system.

That when this fluid is deficient, perverted, or dessicated, it may cause a partial or more complete paralysis, as also atrophic wasting of the muscles, *moisture being so essential for all nervous action.*

That the local pain of gout depends not only upon the irritation set up by the acid state of the blood, but probably also of the acrid fluid circulating through the nervous fibrillæ; and that this hypothesis appears to be considerably strengthened by the development of gouty concretions.

That convulsions, and the undulatory chain of shocks, probably depend upon the abrupt and wave-like motions or impulses communicated to this fluid by the irritated nervous centres.

That the aura in epilepsy slowly, steadily, and continuously, proceeds upwards from the peripheral point of irritation through the nerve to the great nervous centre, where an irrespondent or opposing action in the circulation of the blood probably takes place instantly.

That as in galvanic or electric operations, the very moment the chain is dissevered, the electric or galvanic influence momentarily ceases; so in the animal economy, the very instant an irrespondency takes place as to the regular transmission of the cerebro-spinal fluid, that instant insensibility occurs, to be followed usually by convulsion more or less general and severe.

Such are the opinions I venture, with great diffidence, to submit, as to the uses of the cerebro-spinal fluid. Its importance has certainly not yet been fully recognised. Only recently, when conversing with a distinguished physiologist, he told me he considered "it played but a very subordinate part in the animal economy." I would now, however, fain believe that but few persons will read this synopsis without at least acknowledging that the cerebro-spinal fluid must play a most important part in the animal economy.—*Lancet, Feb.* 18, 1860 *p.* 165.

151.—ON THE RELATIONS OF BELLADONNA AND OPIUM, AND ON POISONING BY BELLADONNA.

By James Seaton, Esq., L.R.C.S.E., L.A.C., Leeds.

[In September, 1858, "two young men having gathered about a pint of the ripe fruit of the atropa belladonna, which they found growing in an old quarry a few miles from Leeds, on their arrival home distributed them among their friends, believing them to be innocuous." A great number of the poisoning cases resulting from this imprudence came under the notice of Mr. Seaton—of which he relates ten—only one being fatal, and concludes by the following remarks.]

The first symptom appears to have been dryness of the mouth and throat; next, indistinctness of vision and dilated pupil; and, afterwards, in the more severe cases, delirium supervened. I found in one man, who had only swallowed one berry, the dry mouth and fauces without any affection of vision. The indistinctness of vision was the most persistent symptom; in all the cases it existed to a greater or less degree for several days, and the boy C. C.'s vision continues defective up to the present time. The delirium was of a busy, restless, vivid character, but generally rather pleasing than otherwise. The patients appeared to think that they were pursuing their ordinary occupations; one boy appeared eager in flying a kite; another pulled chairs and tables about, thinking he was working in a coal-pit; while the woman E. W., appeared to be remarkably busy with her ordinary household duties. All their movements were of a quick, excited character, strikingly resembling delirium tremens. There was no very marked vascular excitement. the skin was, in most of the cases, moderately cool, and the pulse rapid, but without power.

The remarkable tolerance of opium in these cases would appear to bear out the conclusion at which Mr. Bell, Dr. Graves, and others have arrived: that opium and belladonna mutually counteract each other. In none of the cases in which delirium was present, were the symptoms alleviated until sleep was obtained; and, after sleep, the patients felt comparatively well. The pupils were widely dilated so long as the delirium continued, and, when sleep was obtained, were either contracted or reduced to the natural size.

With regard to the fatal case, I may mention, that she was decidedly scrofulous, and had been under treatment for several months for disease of the bones of the face. It will be observed that she took much less opium than several of the cases which recovered, and that, at the moment of death, the pupils were very widely dilated.

The post-mortem appearances are chiefly valuable for their negative testimony. The condition of the brain appeared to be as nearly as possible perfectly natural; as was the mucous membrane of the stomach and intestines. The only thing which was abnormal was the remarkable fluidity of the blood in every part of the body.—*Med. Times and Gazette, Dec.* 3, 1859, *p.* 551.

152.—*On Quinio or Rough Quinine.* By M. Batka.—A substance is known in the Brazils under the name of quinio, which is extracted from the fresh bark of the cinchona by lime and then from the lime by alcohol. It is very rich in quinine, and it is only necessary to boil it with dilute sulphuric acid to obtain an abundant crystallisation of pure sulphate of quinine. Quinio is a yellow body of a resinous appearance and of a bitter taste. It is insoluble in cold and but slightly soluble in boiling water. It is very soluble in alcohol and ether, separating partially from the latter by exposure to the sun. Water precipitates the alcoholic solution. It is almost entirely soluble in weak sulphuric acid, from which soda precipitates it of a dirty white colour, the precipitate assuming the appearance of a resin. A beautiful white sulphate however may be prepared from it. Quinio is free from cellulose; when heated it gives off an odour something like cinnamine, and burnt, leaves a light residue of carbonate of lime. It resembles a good deal the quinoidine of Liebig, but is much purer than the quinoidine of commerce.—*Dublin Hospital Gazette, March* 15, 1860, *p.* 93.

153.—ON THE CHEMICAL AGENTS OF DISEASE IN THE LIVING BODY.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

Animals debilitated by want of proper nourishment submit less readily to the agency of certain poisons than others in a vigorous state of health; but, as you are aware, it has been questioned whether similar modifications are due to nervous influence, and whether the diminished activity of the absorbent powers is not sufficient to explain them. In order to settle the question at once, I injected an aqueous solution of woorara into the veins of two rabbits, one of whom had been previously fasting, while the other was duly fed; in this manner, absorption was entirely dispensed with, the poison being at once conveyed into the blood. The result was such as might have been expected. To poison the fasting animal a dose larger by one-third was required than had been found sufficient to destroy the other. It is, therefore, perfectly clear that all this class of phenomena must be entirely referred to the agency of the nervous system.

But, while the animal is in some measure preserved from the noxious influence of certain poisons, through the rapidly-increasing debility of its nervous system, it becomes obnoxious to the action of morbid influences of a totally different character. It even appears to me, that in our nosological classifications this peculiar liability of the system might be turned to account, as regards the etiology of disease.

To adduce a characteristic instance of this: when frogs have been kept for a long space of time in captivity, their health declines, and ulcerations arise around the nose and mouth; the nervous system being in this case considerably depressed, the animal is of course found to resist much longer the action of strychnia and similar poisons, while parasitical affections spread with fearful rapidity. Frogs are subject to the growth of parasitical fungi, which, after a certain lapse of time, occasion the animal's death. Now, if a healthy frog is placed in a jar, containing others affected with the above-mentioned disease, the new-comer sets contagion at defiance ; while if another frog, affected with ulcerations in the vicinity of the natural orifices, is introduced into the jar, the parasitical vegetation covers it at once.

It has been found that similar affections always have a strong tendency to arise in animals in a low state of health. The itch, a disease which frequently prevails among horses and sheep, is scarcely ever found to attack animals in good condition ; and, in man, the lower classes are known to be a prey to vermin, especially in childhood and old age ; while persons who live under more favourable circumstances are scarcely ever affected with this inconvenience, except towards the latter end of long and painful diseases ; for it is generally in such cases that the *morbus pedicularis* has been observed.

The decrease of nervous power equally constitutes a predisposition to putrid, contagious, and virulent affections ; the fact is well known to veterinary surgeons.

It would appear, therefore, than an apposition exists between the two great classes of disease we have just examined ; in proportion as the animal grows more sensible to the action of neurosthenic poisons, the power of resisting the influence of putrid substances is increased. How is this difference to be accounted for ? We shall attempt to give you a solution of the difficulty.

That the chemical composition of the blood should incessantly be modified, is one of the essential conditions of life ; repairing, as it does, the daily losses of the economy, and renewing the elements of all the tissues which enter into the system, the blood may be compared to a torrent which continually pours out new substances, while other elements are flowing into it ; and the stronger are the animal's vital powers, the more rapid are the successive changes of the blood ; a fact principally observed in birds, which enjoy greater vital energy than any other class of animals. The uninterrupted continuance of circulation is, therefore, in such animals, of still greater importance than in others ; the blood cannot stagnate without promptly acquiring septic properties. If the tributary vessels of a muscle are tied in a mammal or bird, it becomes a putrid mass within twenty-four hours ; in a batrachian this change would not take place before a much longer space of time.

Now, you are aware that the nervous system presides over all the phenomena of life in which motion is concerned ; as soon, therefore, as the nerves are impaired, circulation languishes, and the chemical composition of the blood becomes thereby liable to important changes. If, therefore, an animal being given, it is our purpose to preserve it from the action of woorara, or similar poisons, we must lower its forces. If, on the contrary, we intend to preserve it from contagious diseases, we must increase them by all possible means.

But these septic bodies, or specific poisons, are almost invariably organic substances, and are produced within a living organisation; here we have, no doubt, a peculiar and characteristic biological action; we need not, therefore, be surprised to see pathologists endeavouring to withdraw this class of phenomena from the domain of physiology, in order to make them the exclusive property of medicine.

We must not, however, in my opinion, give up all hope of connecting, one day, these morbid phenomena with the laws of physiology. If at present unable to do so, we shall no doubt succeed at some future period. Is it not, in fact, quite possible that in animals certain physiological conditions may arise, which would give birth to virulent poisons? We are aware that in a perfect state of health, several creatures are venomous; that is to say, they possess a peculiar virus which Nature has given them for the purpose of killing their prey, and defending themselves from their enemies. Here, then, we have a physiological virus; how is it produced within the system? The difficulty is quite as great as with regard to morbid poisons.

It would appear that in several cases the noxious substance prevails throughout the economy; in other cases we only discover it in certain fluids. The virus which occasions hydrophobia belongs to the latter class; it resides exclusively in the animal's saliva. We are not yet aware whether any one of the salivary glands is its peculiar seat, or whether it is indifferently secreted by all of them. No experiments have been tried on this point; but it has been experimentally proved that the peculiar venomous principle does not exist in the blood; transfusion does not convey the disease from a mad dog to a healthy one.

It is a singular fact, and one which pre-eminently deserves our attention, that in so general a disease the virus, which alone is capable of transmitting the affection, should be exclusively localised within one single apparatus, without existing in the blood at large. Yet, if we reflect upon the question, we discover, in the physiological state, a great many similar dispositions; the principles which concur in a vast number of physiological functions; pepsine,. ptyaline, and the active principle of the pancreatic juice, are they not created by special glands? and is not the venom of serpents, which does not exist within the blood, produced by a special apparatus? Viewed in this light a mad dog resembles a viper or a rattlesnake.

But, on the other hand, there exist several virulent diseases, in which the blood really appears to contain the morbid principle. This is the case with the glanders; and it is a well-known fact that healthy animals may be infected with the blood of a deceased horse, as well as with the slimy matter that escapes from the nose and mouth.

But another particular, which will, perhaps, excite your astonishment, is that the normal secretions, bile, saliva, gastric juice, and so forth, do not appear to contain the slightest vestige of this poison; while, on the other hand, the pathological fluids appear to be impregnated with it, and possess the property of transmitting the disease to sound animals—a fact experimentally proved with regard to pus, the fluid contained in a hydrocele, and various other morbid secretions. For this reason alone are the autopsies performed on animals that die of the glanders attended with so much danger; the virus pervades the whole system, and the slightest wound is sufficient to inoculate the complaint.

You need not, however, be astonished at this singular property; you have already witnessed the repulsion which the salivary glands evince for certain substances introduced into the blood; and why should not certain morbid principles be in this manner rejected from all the secretions in which the normal conditions remain unimpaired? The same thing appears to take place with respect to the contagious pneumonia of horned cattle. We are aware that volatile emanations transmit the morbid principle; but experiments have been tried (in Belgium) for the purpose of inoculating it directly to animals, as a preservative against the disease. Something similar to the process of inoculation in the small-pox was expected to result from this; it was then discovered that neither the animal's blood nor any of the fluids of the economy was endowed with the property of propagating the complaint. It appears to have chosen the lung for its exclusive seat, and the liquids therein contained, pus, lymph, etc., are alone endowed with the property of transmitting the complaint. The intense local inflammation which follows the operation sufficiently testifies to the noxious properties of this virus; and when, in order not to spoil the animal's flesh, the tail is selected as the point where inoculation is to be performed, the subsequent inflammation frequently causes it to mortify.

Here, then, we have another virus which exclusively resides in the tissue of the lungs, and is not found in the blood at large; but even in the normal state a great many substances are found in various tissues which do not exist in this fluid. Thus, muscular flesh contains a large amount of salts of potash, while scarcely any trace of them is found in the blood; in a word, the various bodies found in different parts of the economy are not invariably represented in the torrent of the circulation.

The history of specific diseases offers, therefore, nothing which cannot rationally be explained; it now remains for us to discover the physiological process by which a virus may be originated. Nothing is easier than to produce putrid affections in sound animals. Thus, when transfusion is performed under the ordinary conditions—when the blood is conveyed directly from one animal into the veins of another—no accidents whatever are produced; but if the blood is allowed to remain for a short space of time in contact with the atmosphere, and if the serum is then injected into the vessels, all the symptoms of putrid resorption are observed, and the animals die after exhibiting all the characteristic symptoms of putrid infection.

The blood is therefore capable of acquiring toxic properties without the intervention of any foreign principle, merely through the modifications which take place in its composition when life is extinct. The same results may be attained to without even drawing blood from the veins. If the blood of a fasting animal is directly injected into the veins of a healthy one, the latter is poisoned exactly in the same manner as before; and yet the blood, in this case, has not undergone any previous decomposition.

The introduction of foreign principles, of course, acts upon the blood with still more intensity; nearly all the substances known under the name of *ferments*, are endowed with the property of communicating a deleterious influence to this fluid. When yeast is introduced into an animal's veins, passive hemorrhage, and other adynamic symptoms, are immediately produced, and death takes place within a few days. Now, if the animal's blood is transfused into another's veins, all the phenomena previously described take place in rapid succession, exactly as if yeast, and not blood, had been directly poured into the vessels.

It seems likely that in this case a series of decompositions take place within the blood, which give rise to other *ferments*. The well-known experiment related in Pringle's work on Army Diseases, appear to tally with the result of our own experiments.

(In order to prove the influence of putrid emanations, even at as distance, on the chemical phenomena of life, he plunged a thread into the yolk of a rotten egg, and then suspended it in a jar containing the yolk of another egg, and under these circumstances, decomposition took place with far greater rapidity than usual.)

We, therefore, perceive that all this series of phenomena holds intimate connection with that mysterious chemical process known under the name of *catalysis*. The theory of fermentations is at present so imperfectly known—and organic chemistry has in this respect made, as yet, so little progress—that it would hardly be fair to reproach Medicine with its deficiencies on this point. There exists a whole series of diseases which evidently result from the chemical actions which take place within the body. It is, therefore, chemistry alone, which, in its future progress, can teach us the physiological laws which embrace this particular branch of Medicine.—*Med. Times and Gazette, Feb.* 25, 1860, *p.* 183.

154.—THE AIM OF MEDICINE.

By John Cockle, M.D., F.L.S., Physician to the Royal Free Hospital.

The final aim and end of medicine is the cure of disease. The gulf between physiological pathology and therapeutics must be bridged over by clinical observation and experiment; that is to say, by what is known as "rational empiricism." Simple knowledge of the nature of the disease can no more, of itself, suggest the remedy, than can a knowledge of the nature of the rocks and mountains thrown up by volcanic agency suggest the mechanical means by which these may be removed, and the barren spot restored to its primeval freshness. Let us test the truth of what I here advance, by selecting a few of the best known and homeliest illustrations.

Look to the history of the virtues of cinchona bark. The untutored Indian, chance-directed to the healing stream, into which cinchona bark had been driven by the winds, found that the stream that slaked his thirst achieved his cure. And this, to him, was all sufficient. Some reasoning Jesuit traced the pregnant sequence, and added to our therapeutic treasury cinchona bark, one of its richest, most invaluable gifts. Since that poor Indian lived, how many pages have been written, both on the nature of the fever, and the action of the bark! But, I would ask, whether all this so-called knowledge has afforded one single element of explanation of the simple formula—*bark cures intermittent fever?*

Take another illustration. A thousand years ago, the Arabian Rhazes described the pathology of small-pox so accurately, that, even now, after this lapse of ages, it scarcely requires retouching. Barely half a century ago, the protective power of vaccination was discovered. Was this a deduction from the known pathology of the disease? I will not libel your historical knowledge by pausing for a reply. A country milk-maid, living in some remote district, where small-pox frequently raged in all its fearful intensity, learned from simple observation that, if from the udder of a cow, a certain pustule were produced upon the hand, immunity was given from disease. No observation could be more simply empirical than this. The fact in all its simplicity, was related to one of our greatest observers. Jenner's intuitive sagacity saw the deep import of the tale. He followed out the experiment in all its simple grandeur, verified it on the largest scale, and bequeathod to suffering humanity, for all coming time, one of its greatest blessings.

There is a disease called bronchocele, vernacularly *goitre*, or enlargement of the thyroid gland; the cure of which was again discovered by simple observation of the purest kind, though the history is less generally known. It was found that burnt sponge cured the disease; and although more recent researches have shown that the virtue of the sponge was in proportion to the amount of iodine it contained, still the fact is in no way changed, and another practical truth is thus added by simple observation to practical medicine.

I must not dwell on more examples: these sufficiently establish the fact that the remedy here was not deduced from the nature of the disease.

I have now, I trust, more than verified the proposition that clinical experiment is still the most valuable source on which to base the healing art, especially when combined with that rational diagnosis which the sciences named, and that of pathological anatomy, supply. To avoid a cumbrous terminology, I have included the schools of pathological anatomy and physiology; but the above criticism applies with equal force to pure pathological anatomy. The proximate cause of disease may be, and has been, ascertained with the utmost precision by the labours of this distinguished school, and rational diagnosis has been further perfected, but an equal sterility exists with regard to therapeutic indications.—*Brit. Med. Journal, Oct.* 29, 1859, *p.* 874.

155—ON ATROPHY AND DEGENERATION OF THE ARTERIES.

By Edwin Canton, Esq., F.R.C.S., Surgeon to the Charing-Cross Hospital, and Lecturer on Surgical Anatomy.

[The arteries of the brain participate in the degeneration observable in the vessels of other parts of the body, but Dr. Huss has frequently observed in addition to this that]

There is a dilatation of the arteries, both large and small, which ramify in the cerebral substance, so that on slicing the brain they appear of larger size than natural, while the coats of the larger bloodvessels likewise become brittle."

It is well known that an atheromatous state of the arteries strongly predisposes to the occurrence of aneurism; and in regard to the frequency of this latter disease in intemperate persons I may quote the valuable testimony of Dr. Crisp, who observes:—"Aneurism is, I believe, more frequent in England than in any other country. Temperature has but little influence in the production of the disease; but, when the habits of the people are considered—their great industry, the violent and continued exertion they are accustomed to use, as well as their greater addiction to the use of ardent spirits—the cause admits of a ready explanation. Soldiers and sailors, especially, are more liable to aneurism than any other class; and they are called upon, often, when the heart's action is increased by the use of alcohol, to make sudden and violent efforts. Amongst agricultural labourers, on the other hand, who (though subject to great bodily fatigue) generally lead a life of temperance and have their minds free from anxiety, the disease is comparatively rare."

When we remember the condition in which some of the important organs of the body are found after death in habitually intemperate persons; the general implication of the coats of the bloodvessels; the unhealthy condition of the blood itself; and, moreover, the change so frequently to be noticed of the fibres of the heart, can we wonder at the feeble chance of success presented to the surgeon who undertakes the treatment of an anuerism under such untoward circumstances? And, in the abnormal state described of the circulatory apparatus and its contents, can we be surprised at the occurrence of hemorrhage, arteritis, apoplexy, or sudden cessation of the heart's action? Can any condition be more subversive of all those processes whereby the nutritive functions should be duly exercised? The life of the drunkard hangs, indeed, on a thread, and his frail existence may be suddenly cut short by the last contractile effort of a fatty heart, or where the nutritive and reparative powers are so greatly in abeyance, the slightest injury may be rapidly followed by an equally fatal result. Dr. C. Wilson relates the case of a drunkard, in the prime of life, who had been engaged in his usual avocations till a few days previously, when he became indisposed, and was found lying extended on the floor of his apartment, cold and rigid, having evidently been dead for several hours. On assisting his medical man at the post-mortem examination, the liver was found so much enlarged, especially on its upper convexity, that it extended as high as the fourth rib, its whole substance being of a yellow-ochrey colour, and friable in consistence, or in other words, presenting the characteristics of what has been termed the fatty liver. The more immediate cause of death, however, was attributable to the state of the heart, which was *softened, easily lacerable*, and contained fibrinous concretions. Dr. Ormerod has recorded the following instance:—A strong man, between thirty and forty years old, addicted to very hard drinking, received a slight injury of the head. He paid no attention to it, and continued his work for four days; then having headache, and feeling ill, he laid up, and left off all strong drink. In three days signs of delirium tremens came on, and he was brought to the hospital. In the course of the second day after the beginning of these symptoms, while they were pursuing an ordinary course, he took 140 drops of laudanum, but no beer or spirits until the evening, when, the opium seeming to be sufficiently administered, brandy was given him, and beef-tea. In about two hours he went to sleep, and remained dozing and apparently improving in condition all night. But, in the morning, a new nurse came to him, who wished to change his bed-linen. For this purpose she took him out of bed and set him in a chair; but he had hardly been removed when he seemed dying, and he died before he could be again placed in bed. In his body, the only changes were fatty degeneration of the heart and liver. Another case is detailed by Dr. Ormerod, where a man of only thirty-four years of age, of notoriously intemperate habits, and who laboured under anasarca and ascites, died suddenly from fatty degeneration of the heart.

Dr. Watson says: "There is but one morbid condition which, since my attention was directed to it, I have found constant in persons dead of delirium tremens, and that is, a *remarkably soft, pale, and flabby state of the muscular tissue of the heart.*"

I have alluded to the unhealthy condition of the blood, in which Huss states oily particles to abound in that portion of it remaining in the heart and larger arteries in the bodies of drunkards; and this fluid, when rubbed between the fingers, gives the same feeling as though it were fat. This increased amount of fat in the blood (*pionœma*) is said by Wedl to confer on the serum a whitish, milky discoloration, and the floating fat-globules may be easily recognised under the microscope.

The blood, in addition to presenting these peculiarities in drunkards, is found to be defective in its usual amount of fibrine, and a lower than the normal grade of plasticity prevails: the corpuscles appear to be diminished in number, and the quantity of the aqueous parts is increased; there is less than the ordinary estimate of hæmatine present, and the carbon is superabundant. Scharlan has found as much as 30 per cent. more carbon in the blood of a drunkard than in that of a healthy man.

Beyond the fact of the direct passage into the blood of the alcoholic fluid received by the stomach, the accumulation of hydro-carbonaceous matter in it may be accounted for by the diminution of the amount of carbonic acid and water expelled from the lungs by those under the influence of this stimulus. "But," observes Dr. C. Wilson, "physical, and even mental exertion, increases the degree of exhalation of carbonic acid by the lungs, and therefore operates also by increasing the power of resisting effects. Hence the drunkard, reduced to inaction, loses yet another of his grounds of vantage. Fasting, on the other hand, causes less carbonic acid to be exhaled, and fasting increases the proneness to intoxication and the mischief of its results."

The general diseased state induced in persons addicted to intemperance has been so jealously cared for by those interested in life assurance, that statisticians have made the rate of mortality in regard to them an object of special attention: and Neison has found that at the term of life from twenty-one to thirty, or in the first vigour of manhood, the mortality of drunkards was upwards of five times, and from thirty-one to fifty upwards of four times, that of the general community of like ages; and he remarks—"If there be anything in the usages of society calculated to destroy life, the most powerful is, certainly, the inordinate use of strong drinks."

"It cannot be too widely known throughout the length and breadth of the land," observes Mr. Smee, "that though during the last few years the most severe competition has taken place between different life assurance companies for business, not one has dared to insure intemperate lives, or lives which have been intemperate, at any increased rate. No reasonable rate meets the case. It is in vain that the drunkard turns to propriety; he can never altogether erase the mischief; and although much may be done by great perseverance and skill, he never can altogether bring himself into a healthy state, or be regarded as a thoroughly healthy life. *The frame of the drunkard is debilitated throughout*, and he sinks by pneumonia, disease of the liver, or other malady depending upon debility, which cuts off his career prematurely. Insurance companies abhor the drunkard, as a risk far too great and uncertain to be estimated and safely considered.

If the career of the drunkard be watched, it is astonishing how soon he passes away, as if the voice of Nature exclaimed, 'Cut it down! why cumbers it the ground!'"

In the cases I have related, where there was found after death marked degeneracy of the heart and arteries in the bodies of drunkards, and corresponding change in other organs, there existed in the corneæ well-developed circles of fatty degeneration; and when we consider the great involvement of the other tissues in this alteration, and the degraded nutrition of all, it might, not unreasonably, be anticipated that the eye will afford corroborative testimony to the general evidence of ravages so wide in extent and deadly in form—ravages which have rapidly reduced youth to age, and are hurriedly leading the "artificial old man" to premature decrepitude and the tomb. "The arcus, or circulus senilis of the eye," observes Dr. C. J. B. Williams, "almost always indicates premature age, brought on by the wear and tear of extensive mental anxiety, or by *fast living*."—*Lancet, March* 17, 1860, *p.* 269.

156.—ON EGYPT AS A RESIDENCE FOR EUROPEAN INVALIDS.

By Dr. W. Reil.

The principal difference between Cairo and Madeira appears to consist in this, that in Cairo the air is very dry, and the thermometrical oscillations are pretty considerable during the winter months; whilst in Madeira the air is very moist, and the variations of temperature are very trifling. Hence it may be asked, how can phthisical patients recover their health under such different circumstances; and is there really no difference in the mode of operation of such very different external agencies? Dr. Reil confesses that statistical evidence is wanting, especially in the case of Cairo, to enable us to give a complete answer to these questions. The following is, however, his individual opinion, which agrees with that of a large number of medical men who have been for a time settled in Egypt.

An acute form of phthisis, with intense fever, frequent short cough, great dyspnœa on exertion, expectoration scanty but often sanguinolent: such are the symptoms which appear to indicate a residence in Madeira; whilst patients in whom the pulmonary disease is running a chronic course, who are subject to exacerabations followed by ameliorations, in whom the expectoration is abundant, not accompanied with hæmoptysis, are very speedily benefited by the dry atmosphere of Cairo. In cases where we can only anticipate the gradual sinking of our patients, when we would leave them to die in the open air, as it were, Madeira should be preferred, were it only for the superior comforts which that island affords.

At the first onset of pulmonary tuberculosis, when the patient is young, while his constitution is still strong, and his powers of assimilation are normal, Dr. Reil recognises no difference between Cairo and Madeira. It is certain that the air in the former of these localities, on account of its great dryness, its purity, and its comparatively lower temperature, may act as a tonic and excitant; whilst, on the other hand, the warm and moist air of Madeira is suitable to the irritability of certain temperaments, although in itself it has also a relaxing and debilitating action.

Dr. Mittermaier has found almost invariably, on examining the bodies of phthisical patients in Madeira, hyperæmia and granular degeneration of the kidneys, the latter condition often to a high degree. But, says Dr. Reil, this pathological condition is not so constantly found accompanying pulmonary phthisis as to warrant us in considering it an integral part of the disease, even if we admit a physiological relation between the two affections. We are warranted, then, in expressing the suspicion that the renal affection has been determined by the influence of the climate; on this most favourable hypothesis, it has certainly not been cured by it. On the contrary, Dr. Reil appeals to his fellow-practitioners in Egypt, Drs. Reyer, Lautuer, and Bilharz, to confirm his own observations, that considerable proportions of albumen detected in the urine of phthisical patients recently arrived from Europe, disappear during their residence in Cairo or Upper Egypt, and in some *post-mortem* examinations kidneys have been found presenting the appearance of cured granular degeneration. There is also no doubt that, at the autopsies of phthisical patients who have died in Cairo of some acute intercurrent affection, large cavities have been met with, lined with a smooth false membrane, and consequently in progress of cure, as well as smaller vomicæ already in process of conversion into a dense cicatrix tissue.

On one point all the medical authorities are unanimous, that, even at the very commencement of the disease, a cure cannot be effected, except on condition that the patients reside for two or three winters in Cairo or Madeira; and that for a more advanced stage of the disease, with infiltration and formation of cavities, the only chance of success is in a permanent abode in the country.

Far more certain than the cure of pulmonary phthisis either in Cairo or Madeira, is the cure of affections of the respiratory passages not connected with the tubercular diathesis. Such are, chronic bronchitis, which has followed or has accompanied pleuritic effusions, chronic laryngitis, and emphysema. In these cases the choice between Cairo and Madeira will depend upon the constitution of the individual patient, and in the special effects required from the climate in each.

There are other diseases characterized by a condition of asthenia, in which a residence in Egypt is indicated; such are anæmia, hydræmia, and chlorosis. It is scarcely necessary to add what good effects may expected in the case of patients convalescent after severe fevers or abundant hemorrhage.—*Gazette Hebdomadaire.—Edin. Med. Journal, April,* 1860, *p.* 959.

157.—*Antimony and its Detection.* By MM. LASSAIGNE AND LORAIN.—M. Lassaigne, in commenting on a case in which it was necessary to discover whether antimony were or were not present in the tissues of a person supposed to have been poisoned by successive doses of emetics, makes some objections to the use of Marsh's apparatus. He argues that, after complete oxidation of tissue, the antimony passed over from the Marsh bottle in the form of antimoniuretted hydrogen, only represents a part of the absolute amount contained. In this respect, Marsh's process contrasts very unfavourably as compared with its application to the detection of arsenic. The following is a description of the more simple plan for the qualitative detection of antimony in an organ, as the liver.

MM. Lassaigne and Lorain burned fifty *grammes* of the liver in a new porcelain crucible. The carbon proceeding from the burning was calcined by maintaining during several hours a cherry-red heat. The ash collected, reduced to a fine powder, was successively treated with weak nitric acid and pure hydrochloric acid. The solution, to which was added a solution of weak tartaric acid, was filtered and brought into contact with three times its volume of hydrosulphuric acid. This reagent immediately threw down a yellowish orange flaky precipitate of hydrated sulphide of antimony, the volume and colour of which, compared with a precipitate formed in a standard solution of tartar emetic, permitted the operators to estimate the proportions of the salt contained in the hepatic tissue submitted to observation.—*Ann. d'Hygiène publique.—Brit. and Foreign Medico-Chirurgical Review, April* 1860, *p.* 527.

158.—DETECTION OF ARSENIC, ANTIMONY, COPPER, AND BISMUTH, BY ELECTROLYSIS.

By PROF. BLOXAM.

At the last meeting of the Chemical Society, Professor Bloxam, of King's College, gave the results of a very successful investigation of this subject, and showed that, by proper refinement in the method of operating, the process of electrolysis may become a certain and delicate means of detecting one or all of the metallic poisons, at least, with but few exceptions. In an examination for arsenic by this method, the metal is obtained in the form of arseniuretted hydrogen; the process is therefore very similar to that of Marsh, over which, indeed, it does not present any advantage in point of delicacy. Marsh's process, however, although it is capable of doing all that can be done by electrolysis with even greater delicacy, is open to several well-known objections, which have stood in the way of its practical adoption by toxicologists. The process of electrolysis does not involve the use of zinc, which is so difficult to obtain pure. It forms a general method for the detection of several metallic poisons at once, and the material tested is not destroyed or inconveniently contaminated, but may be used for another operation. When the arsenic, on the other hand, is present in a state of arsenic acid, it cannot, according to Prof. Bloxam's experiments, be detected with certainty by electrolysis. It is consequently necessary to reduce the arsenic acid by means of sulphurous acid. This is an objection which does not apply to Marsh's process. In his earlier experiments, Mr. Bloxam made use of a U tube containing dilute sulphuric acid; the substance to be tested was introduced into one of the limbs, and a cork with a bent tube fitted to its mouth; two platinum plates, leading from the poles of a battery containing five cells of Groves, were introduced into the two limbs, and the liberated hydrogen passed through the bent tube, which was heated by a lamp, when the arsenic, if present, was deposited.

The form of apparatus ultimately adopted as being the most convenient, consists of a two or three-ounce bottle, the bottom of which has been cut off, and replaced by a piece of vegetable parchment, bound on with platinum wire. To the mouth of the bottle is fitted a cork with a bent tube and a piece of platinum wire, which passes through the cork, and turns up beneath in the form of a hook. A slip of platinum then hooks in the end of the wire, and passes nearly to the bottom of the bottle; it forms the negative pole of the arrangement. The bottle stands in an ordinary test-glass, and the positive pole, also of platinum, stands in the glass. Dilute sulphuric acid is put into the bottle, and also the glass, so as to stand to the same height in both vessels. The substance to be tested is introduced into the bottle, the cork adjusted, and the wires connected by five cells of Groves' battery; the heat of a spirit lamp is applied to the bent tube, and in the course of a quarter of an hour a distinct mirror is obtained, if arsenic is present. Standard solutions, containing respectively a tenth, a hundreth, and a thousandth of a grain of arsenious acid, were prepared and examined by this process, and in every case a successful result was obtained. These solutions were then mixed with organic substances, such as the ordinary articles of food—meat, eggs, milk, &c.—and the resulting matter examined.

It was got into solution by means of chlorate of potash and hydrochloric acid, and the resulting fluid evaporated down by means of a water-bath to a thick, syrupy liquid. The arsenic was thus obtained in the state of arsenic acid, which does not give a certain result by the electrolytic process. Some sulphurous acid was therefore added, and the mixture introduced into the bottle, after expelling the excess of sulphurous acid by evaporation; a drachm of alcohol was then poured over the surface, and the process put into operation. The author prefers to add to this drachm of alcohol in every case, inasmuch as it not only allays the frothing, but also affords an additional indication of the presence of arsenic; for when these two substances are present—the alcohol and the arsenic—the gas which escapes at the open end of the test tube possesses a very peculiar odour, resembling alkarsin. If a little sulphurous acid be present, it also furnishes an additional character indicative of arsenic ; namely, a slight yellow deposit, consisting of sulphide of arsenic, close to the borders of the metallic mirror. In all these experiments, of which a great number were made, the thousandth of a grain of arsenious acid was readily detected.

The other metals which may be detected by this process are mercury, antimony, copper, and bismuth ; lead is precluded by the sulphuric acid which is present. These are all precipitated in the metallic form upon the slip of platinum. and even in the case of antimony a mere trace of antimoniuretted hydrogen is formed, the metal being all deposited upon the negative pole. The mode of proceeding in these cases is precisely similar to that adopted for arsenic ; when the operation is concluded the slip of platinum is detached, washed, and the deposit dissolved off in the usual manner. Thus, where an organic mixture has to be examined for arsenic, mercury, copper, antimony, and bismuth, it is prepared in the manner just described for arsenic, and the resulting liquid introduced into the bottle, the drachm of alcohol poured over the surface of the contents, the cork adjusted, and the battery connected. The heat of a spirit lamp is applied to the bent tube, and the operation continued for about a quarter of an hour or twenty minutes, when, if arsenic is present, a metallic deposit, accompanied by some crystals of arsenious acid, will be formed in the tube, and the escaping gas will have the alkarsin-like odour. The piece of plantinum in the bottle is next removed, washed, and boiled in yellow sulphide of ammonium. Antimony would be dissolved and might be obtained as sulphide by evaporating this solution to dryness. The other metals would still remain in the plate; it is next boiled in nitric acid containing a trace of hydrochloric acid, the solution evaporated to a small bulk, and an excess of ammonia added. Oxide of bismuth would be precipitated, together with whatever traces of platinum had been dissolved. The precipitate may be dissolved in hydrochloric acid, and tested by pouring into water, &c. The ammoniacal filtrate would contain the copper, indicated by its blue colour, and the mercury. By boiling with hydrochloric acid and a slip of copper, the latter would be separated in the metallic form.—*Pharmaceutical Journal, Jan.* 1860.—*Brit. and For. Medico-Chirurg. Review, April* 1860, *p.* 527.

ABSTRACTS FROM WORKS PUBLISHED DURING THE PAST SIX MONTHS.

[Intended, as this Work is, for the perusal of the large mass of medical men, whose time is too fully occupied to read all the Journals and Works published, we shall, in the following abstracts, chiefly notice those parts of works especially bearing upon the *treatment* of diseases.]

159.—*Clinical Lectures on Certain Acute Diseases.* By ROBERT BENTLEY TODD, M.D., F.R.S., Physician to King's College Hospital.—*London*, Churchill, 1860. pp. 487.

[The design of the lectures published in this volume is to illustrate the clinical history and treatment of the more important acute diseases. The principal diseases treated of are Rheumatic and Continued Fevers, Erysipelas, and Pneumonia. In the preface, the conclusion to which the observations detailed in the lectures tend more or less to establish, are thus summed up:]

1. That the notion so long prevalent in the schools, that acute disease can be prevented or cured by means which depress and reduce vital and nervous power, is altogether fallacious.
2. That acute disease is not curable by the direct influence of any form of drug or any known remedial agent, excepting when it is capable of acting as an antidote, or of neutralizing a poison, on the presence of which in the system the disease may depend (*materies morbi*).
3. That disease is cured by natural processes, to promote which, in their full vigour, vital power must be upheld. Remedies, whether in the shape of drugs, which exercise a special physiological influence on the system, or in whatever form, are useful only so far as they may excite, assist, or promote these natural curative processes.
4. That it should be the aim of the physician (after he has sedulously studied the clinical history of disease, and made himself master of its diagnosis), to inquire minutely into the intimate nature of these curative processes—their physiology, so to speak—to discover the best means of assisting them, to search for antidotes to morbid poisons, and to ascertain the best and most convenient methods of upholding vital power. (p. vi.)

[We extract Dr. Todd's observations on the treatment of *rheumatic fever.* He considers that as many as seven different plans of treatment may be specified, all at one time or another having strenuous advocates. The first plan is that by venesection:]

It was formerly the prevailing opinion, and it is still, unfortunately, thought by some, that when called to a case of rheumatic fever one had only to open a vein, and if he could succeed in taking away a sufficient quantity of blood, which, in many instances, it was laid down should be little short of one or two pints, that by this large and rapid abstraction of blood, the disease may be cut short, and a malady, which ordinarily lasts some weeks, may be converted into one of a few days' duration. (p. 27.)

[Now, although by this mode of practice, frequently the more urgent symptoms are apparently quickly overcome, yet patients so treated linger on for considerable periods, suffering much from chronic rheumatism, and exhibiting an extreme anæmia, from which they but slowly, if ever, recover. The number of supporters of this system is very small at present.]

It is a practice from the adoption of which I would most earnestly dissuade you, as having the support neither of reason nor of experience, and as being fraught with the most dangerous consequences to your patients. (p. 28.)

The second plan of treatment is that by moderate bleeding and diaphoretics. This may be called an "expectant" treatment; but it is more than that as regards the venesection; while in other respects it sufficiently merits the name. The advocate of such a plan will say—"When I am called in to a case of rheumatic fever, I think it advisable to commence the treatment by abstracting about ten or twelve ounces of blood, and then to give sudorifics and purgatives."

Now, the objection which I entertain to such treatment is this:—that the routine abstraction of blood can scarcely be called *necessary* in any case, and that in many it is injurious. The tendency of rheumatic fever is to impoverish the blood, especially as regards that highly important portion of it, its colouring matter. All that bleeding really effects is to relieve pain (which, however, may quickly return, for a few hours, while it undoubtedly aids the bleaching power of the rheumatic matter, and, as I have observed in several cases, it increases much the tendency to a chronic rheumatic state, and consequently prolongs the convalescence. That bleeding in rheumatic fever is unnecessary, and that its omission diminishes rather than increases the tendency to certain internal inflammations, I am so convinced, that for several years I have not abstracted blood, in any way, in a single case of the disease. The treatment of rheumatic fever by the abstraction of blood, even in moderate quantity, but more especially in large quantity, appears to me to increase the danger of internal effusions into the pericardium and the pleura, and also into the synovial sacs of the joints. Under this treatment we also meet with the most violent and troublesome cases of delirium, which, under other methods, either does not occur, or is developed in a form sufficiently easily controlled. I am very much disposed to believe that this treatment predisposes to pericarditis and endocarditis; and that, if these affections occur in a case in which venesection has been freely practised, they are much less tractable than when you have to deal with them in a patient who has not suffered from loss of blood.

A third plan is that by mercury. Some recommend that calomel and opium should be freely administered until salivation is produced. The great objection to this treatment is, that it is an attempt to cure one fever by setting up another, and, in some respects, a worse: even supposing the original disease succumbs, your patient comes out of his rheumatic fever with loose teeth, ulcerated gums, and all the painful and offensive concomitants of ptyalism. Now, I say, that under such circumstances the remedy is nearly as bad as the disease; and, moreover, it does not in the least guard the patient against what may be termed the accidents of his malady—those severe internal inflammations—pericarditis, endocarditis, pneumonia, pleuritis, peritonitis. I have more than once seen pericardial inflammation supervene while the patient was in a state of salivation, of which the Case (2) of Sarah Green, detailed in the first lecture, is a good example. When we consider how differently various persons are affected by a mercurial course, and how much some suffer from it, even if given in small quantity, it would seem highly inexpedient to adopt this plan of treatment, for it assuredly offers no prospect of effecting either a speedy cure or a speedy convalescence, much less both together.

It is worthy of remark that rheumatic patients sometimes exhibit a distinct tolerance of mercury, and are with difficulty salivated. The following case is an instance in point, affording, at the same time, but little encouragement to the supporters of the mercurial treatment, and yielding no evidence of the anti-rheumatic power of mercury. (p. 39.)

[A fourth plan of the treatment is by colchicum and guaiacum. Colchicum especially has long been considered to possess a specific influence over rheumatic and gouty affections. The influence of this drug in gout is another question; but in rheumatism it is of no use. Guaiacum has even less claim. Even in these few cases in which any good effect may follow their administration, a degree of prostration and debility is produced which is more dangerous than the original disease.]

5. Treatment by Opium.—This plan of treatment has been lately revived by a very able physician, Dr. Corrigan, of Dublin. It has much to recommend it, and, on the whole, you will find it extremely serviceable in practice; but I do not recommend it alone: its great value consists in relieving suffering, and soothing the nervous system, while it promotes diaphoresis. The opium is given in large and frequently repeated doses, care being taken not to produce too much narcotism; but upon this point, in general, there is not much need for fear, as there seems to be in the generality of patients, a remarkable

tolerance of opium. Our patient, Elizabeth Stocking, whose case I have described in the last lecture, was ordered on the 23rd a grain of opium, to be given every three hours, in addition to half a grain of the muriate of morphia, which she had previously been taking at night: in forty-eight hours she thus took sixteen grains of opium, exclusive of the morphia, yet her pupils were not at all contracted, nor was she in any degree narcotized. The effect upon her has been most beneficial: —her nervous excitement has been calmed down, and her pain materially relieved. The same plan was pursued in the case of S. Green, also detailed in the last lecture: she had one grain of opium every three hours from January 8th to February 10th, excepting for one week, in which the dose was reduced to half a grain; the same tolerance of the remedy was observed. It will not, however, do to employ this plan alone: it should be conjoined with other treatment. I do not recommend it by itself.

I may here adduce another case in illustration of the benefit to be derived from opium, where there is much disturbance of the nervous system with restlessness and delirium. (p. 48.)

A sixth plan of treatment, proposed long ago, by Dr. Haygarth, consists in giving bark in large doses, for which, more recently, the less bulky sulphate of quina has been substituted. Now just imagine the state in which the pathology of a disease must be, when measures so completely at the opposite extremes of our therapeutical resources are advocated for it—as venesection, to the amount of two or three pints, on the one hand, and large doses of quinine on the other; some would even give as much as five or ten grains two or three times a day. Now I have tried both methods of treatment, and I approve of neither; but if I were tied down to one or other of them, I should not hesitate to choose that by bark. In cases where the sweating is colliquative, and the urine copious and pale, with abundant precipitates of *pale* lithates, I have seen great good done rapidly by the use of quinine; but I am not prepared to advise you to adopt this treatment from the beginning, because it tends to check secretion, and so may favour the development of internal inflammations.

The seventh and last mode of treatment that I shall mention to you is, that which you have seen me adopt frequently at this hospital, namely, *the treatment by elimination*. I give it this name, in order that you may keep well in view its main object—to promote the elimination of morbid matter by the various emunctories, and also that you may bear in mind the view of the pathology of the disease upon which it is founded.

It is probable that *the materies morbi* in rheumatic fever is lactic acid or some analogous agent. We know that the natural emunctory of this is the skin. Many chemists maintain that it will also escape by the kidneys; and if it ever does so, perhaps this is more likely during rheumatic fever than at any other time. Again, since vitiated digestion is apt to produce it in undue quantity, and it, therefore, is formed abundantly in the stomach, there is every reason to think a certain proportion of it may be carried off through the alimentary canal. The indications are, then, to promote the action of the skin, the kidneys, and the bowels; to use antacid remedies; and to give large quantities of fluid for the free dilution of the materies morbi, and to supply the waste caused by the drainage from diaphoresis and diuresis.

The best way to promote the action of the skin is by opium, especially if you combine with it nitre and ipecacuanha. For this purpose I sometimes use a compound which resembles the original Dover's powder, in containing nitrate of potass instead of sulphate of potass, as prescribed in the compound ipecacuan powder of the Pharmacopœia. Our usual prescription is one grain of opium, one grain of ipecacuanha, and five grains of nitre; this must be given every two, three, or four hours, according to the urgency of the symptoms, and the need the patient has for opium. This drug quiets the nervous system, and procures sleep, and with the ipecacuan promotes sweating; while the nitre acts upon the kidneys, and the ipecacuan may exercise some influence on the liver.

The best alkali on the whole is the bicarbonate of potass, which may be given in large and often-repeated doses—a scruple or half a drachm every third hour. Sometimes the acetate of potass answers very well in similar doses, and many physicians much prefer it to any other alkaline salt.

Next you must give purgatives to such an extent as to keep the bowels in a loose state, taking care not to carry this treatment so far as to weaken your patient, or worry him by obliging him to be frequently moved in and out of bed. You will find it advantageous to use an alkaline purgative; and there cannot be a better medicine for this purpose than our hospital nostrum—the white mixture containing magnesia and sulphate of magnesia. Sometimes you may give the potassio-tartrate of antimony with advantage; but as it is a depressing remedy it is seldom advisable to use it.

But while we are thus alkalising our patient, and giving internally sudorifics and diaphoretics, ought we not to attend to the state of the joints? The diligent physician will tell you by all means to attack them at once:—but there is such a thing as "nimium diligentiæ" in physic as well as in other matters. Many will say, the best thing you can do is to leech a painful and swollen joint; I formerly tried this practice extensively, but for some time past I have not done so, as I generally found it either useless or injurious. You may apply leeches, and in a short time after you will find the pain and swelling removed, and you may be disposed to say, "here is a proof of their efficacy;" but wait twenty-four hours, and then you will generally find the pain and swelling as bad as ever, and the joint in just the same condition as before. Now apply leeches, and you will probably fail to give any relief. You have by the first application relieved the pain for a time, but you have produced no permanent good—you have rendered the disease more erratic, and less amenable to subsequent treatment. Frequently, when you leech a joint, the pain and swelling subside, but its fellow becomes swollen; leech it, and the swelling and pain return to the original joint. Nothing is more important to avoid, nor more troublesome if not prevented, than the erratic tendency of the rheumatic state. It will fly from joint to joint, and in pursuing it with leeches you will only drive it out of one joint into another. I am satisfied that leeching the joints favours this erratic tendency.

I am not prepared, however, to advise you to neglect the local treatment of the joints. When they are much swollen and painful, you may give great ease to your patient by enveloping them in a large quantity of the soft carded cotton — commonly called *cotton-wool.* Over this you must wrap a sheet of oiled silk, so as to cover in the wool completely, taking care to have no part of it exposed. By this air-tight covering you keep the joints in a complete vapour bath; and when you come to remove the oiled silk and wool, after twelve or twenty-four hours, you find the wool completely saturated with moisture, which generally is strongly acid. You have seen this in Elizabeth Stocking's case. We find the plan so generally useful, that it is adopted in the hospital in nearly every case; it affords great relief, supports and keeps the limb steady, and at the same time promotes sweating. I may just mention, that this plan of enveloping the joint in wool and oiled silk is also very beneficial in gout.

In a few, and only a very few, cases, I have found the pain aggravated by the heat which this mode of wrapping generates; and in cases where it is desirable to keep down the sweating, it is not advantageous to carry this plan beyond a day or two.

The best additional local treatment is that by blisters of small size, applied on or near the affected joints; they are very useful both in acute rheumatic and acute gouty joints. I shall refer to this subject again.

You perceive that all the means employed in this mode of treatment tend to elimination, and to the relief of pain; the opiate sudorific affecting the skin; the nitre and alkaline salts acting on the kidneys; the purgatives on the mucous membrane of the bowels; the wool and blisters on the joints.

During this treatment, while you allow your patients the liberal use of simple diluents, you must give a fair amount of nourishment from the first; and I think this may be best supplied by a small quantity of good beef-tea, given frequently throughout the day. *(pp. 49—54.)—Dr. R. B. Todd.*

160.—*The Causes and Treatment of Imperfect Digestion.* By Arthur Leared, M.B., M.R.I.A., Physician to the Great Northern Hospital.—*London*, Churchill, 1860. pp. 224.

[From this work, which is well worthy the perusal of our readers, we extract the following on the efficacy of strychnia in the treatment of dyspepsia.]

Strychnia acts not only as a bitter, but possesses other valuable properties in dyspepsia.

It need hardly be urged that this energetic drug requires to be cautiously administered, but its effects will amply repay the care. Speaking from extensive experience, I know no single medicine of more value in this disease. Strychnia is particularly indicated in dyspepsia attended with nervous debility. In that numerous class of cases in which abnormal sensations in various parts of the body—as the throat, the head, or the limbs—are experienced, it will generally be found useful. It is the best tonic for the class in which mental symptoms predominate. But it also possesses excellent local effects, and acts by increasing the tone of the muscular coats of the stomach and intestines. When these coats are relaxed, gases are generated, mainly owing to retardation of the aliment in the cavities. No remedy has in my hands proved so permanently effectual as strychnia against this inconvenience. In the case of a gentleman who suffered most severely from sudden and almost daily accumulations of gas in the stomach and bowels, these attacks were attended by great mental oppression; often by fits of crying. The symptoms, in fact, resembled those of hysteria very closely. I mention the case particularly on account of its severity, and because the patient was cured by strychnia; and some time has now elapsed without a return of the attacks.

Strychnia should always be given in solution for the sake of its more effective diffusion in the stomach, and for the sake of greater certainty in apportioning the dose. A proportion of some acid, as a drachm of dilute acetic acid to a grain of strychnia, should be prescribed with it, as the action of strychnia is greatly favoured by association with acids. The discrepant accounts given of it, in all probability depend on different degrees of acidity in the stomachs into which solid strychnia was introduced. As a bitter tonic, the fortieth part of a grain is a sufficient dose. When it is desired to insure specific action, the twentieth part of a grain may be given; but it will seldom be necessary to exceed this, as the good effects of strychnia on the gastro-intestinal muscular fibres are usually secured by a quantity that does not affect the voluntary muscles. A solution of strychnia of definite strength that will keep, which that made with acid will not do, is a desideratum in the forthcoming national Pharmacopæia.

A preparation of citrate of iron and strychnia has lately been introduced, but it has no claim to be regarded as a chemical compound. It is simply a useful mixture; but the variable proportions adopted by the makers, within my own knowledge, ranging from one part of citrate of strychnia in ninety parts. to one part in one hundred and fifty, is highly objectionable.—*Dr. Leared, p.* 185.

161.—*Hemorrhoids and Prolapsus of the Rectum, with especial Reference to the Application of Nitric Acid.* By HENRY SMITH, F.R.C.S., Surgeon to the Westminster General Dispensary; formerly House Surgeon to King's College Hospital.—*London,* Churchill, Second Edition, 1860. pp. 108.

[Various papers have at different times appeared in our pages, upon the treatment of Hæmorrhoids by the application of Nitric Acid. The following gives. in a very clear light, the class of cases in which this mode of treatment is especially useful, distinguishing them from those cases in which the use of the ligature is more suitable.]

I have stated, whilst describing the nature of internal hæmorrhoidal diseases, that the condition in which they are found varies. I have endeavoured to point out those cases to which the operation of the ligature is applicable, and have stated that this practice is necessary and justifiable in those instances where the tumours are large, mainly composed of tissues in which the veins predominate. and have become indurated. There are, however, other instances when the hæmorrhoidal tumours are small or moderate in size, and where they are evidently composed of morbid texture, in which the small arteries rather than the veins are interested, as shown by their bright florid aspect, and their tendency to pour out arterial blood whenever the patient is at the closet, or when the tumours are handled. These tumours are generally not very prominent. They produce exceeding annoyance, and indeed, prove more destructive to the health, as they generally yield a great deal of blood. Now, in such cases the ligature will undoubtedly be as effective as in the other instances before described; but this proceeding is not necessary, as the local use of the nitric acid is so eminently suited to them. The relief which one single application of the acid gives in these cases is remarkable, and an excellent cure may be effected, if the whole of the diseased texture be subject to its action.

About these particular kind of cases, there is no doubt in the mind of any surgeon who has seen the nitric acid applied in a proper manner. There is however a mixed class of cases where the remedy is an uncertain one, but in which, nevertheless, the surgeon is justified in trying it, and where I sometimes have succeeded when I little expected it. I refer to those cases where there is a hæmorrhoidal mass, consisting perhaps of one tumour, mainly composed of venous ramifications, and of a bluish colour, with one or more presenting the characters of the florid sessile pile; or one portion of the tumour or tumours may present the dark blue appearance and thickened membrane, and another portion of it may be brightly vascular, have its mucous covering granular and slightly ulcerated. In this kind of mixed case, I do not hesitate to try the acid if the patient is particularly desirous, but I make a point of stating that it is impossible to depend upon any curative action in such, although in some instances the remedy has acted most efficiently.

To apply the agent to those cases where the tumours are large and indurated, and have a deep blue colour, would be perfectly useless, and only bring discredit upon the nitric acid as a means of cure in other kinds of hæmorrhoidal disease. I particularly wish to point this out, as it is very probable that some of my brethren think that I am an enthusiast with regard to the use of this remedy, and recommend it too strongly. On the contrary, I always urge upon the patient the necessity and propriety of the ligature in such, and even in the more doubtful case just alluded to. It is not necessary, because a surgeon may have reason to adopt, with confidence, a particular kind of remedy, that he should be an enthusiast, or should be blind to the value of those other means which are generally recognised as suitable and efficient.

It is however, in that class of cases not unfrequently met with, where there is not so much any decided hæmorrhoidal tumour, but where there is a generally congested and relaxed condition of the mucous membrane of the rectum, attended with bleeding to a greater or less extent, that the nitric acid acts so beneficially. Dr. Houston has compared this condition of the rectum not inaptly to that of the thickened conjunctiva after long-continued ophthalmia. The application of the acid to the diseased points from which the bleeding proceeds, will soon remedy all the bad symptoms.

Having described the cases to which this agent is applicable, it is fitting that something should be said about its mode of action.

When the strong nitric acid is applied in a very limited degree to the vascular mucous membrane, or granular tumour, I believe it acts beneficially, much in the same way as does the nitrate of silver, or other powerful caustic, when applied to a spongy, irritable sore on the leg, by altering the condition of the minute vessels contracting them and perhaps causing coagulation of the blood in them. When however it is used more freely, superficial destruction of the tissue to which it is applied takes place, an eschar forms, this is in time removed, cicatrization necessarily ensues, vessels which formerly bled are closed up, the tissues generally are braced up and contracted, and probably adhesion is formed between the mucous and muscular tissue of the gut. By further applications of the acid to the same part, the morbid texture may be actually destroyed; and hence the remedy, powerful as it is, requires great care in its use.

I will now describe the manner in which the acid should be applied.

The bowels having been well evacuated some hours previously, the diseased portion to which the application is going to be made should be well exposed, by making the patient sit over hot water for some few minutes; or, if this is not sufficient, an enema of water should be thrown up the rectum, and the hemorrhoidal disease will be brought well into view. The part to which the acid is to be applied, should then be carefully wiped with a portion of lint. The surgeon then dips the extremity of a small, flat piece of wood into the nitric acid, and touches the diseased surface carefully with it. The part touched, and the neighbouring mucous membrane is well smeared with oil, and the whole of the exposed part is returned within the orifice.

There are certain details connected with the application of this agent which require attention. Thus, the acid which is used should be the strongest and purest which can be obtained. I have generally employed the colourless acid, but sometimes I have thought it desirable to use the brown *fuming* nitrous acid, which acts more energetically. I have used for the most part a piece of wood as the means of carrying the acid on to the part, but some object to it as liable to be acted upon by the agent, and therefore recommend a glass rod or brush, both of which suffice very well, but I do not think it much matters.

It is important to touch the part only lightly, especially in those cases of vascular piles where bleeding easily takes places; for the blood escaping and mixing with the acid, will in a measure neutralize its effects. I also lay great stress upon the necessity of wiping the diseased part with lint, previous to applying the acid, for there is always a certain amount of mucus covering these tumours, and unless this be removed, the nitric acid will not act efficiently.

I have recommended that the diseased parts should be brought down either by the action of an enema, or by the efforts of the patient sitting over warm water; but there are cases where it will be difficult and unpleasant to effect this object in either way, and in this case I adopt the plan recommended many years since by Mr. Fergusson, of using a silvered glass speculum, by the introduction of which a good view of the hemorrhoidal tumour may be had, and the nitric acid can be readily applied to the part through the instrument, into the eye of which the diseased tissue is by a little mangement easily engaged.

[In these cases we have usually employed Weiss' female urethra dilator, (a wood-cut representing a modification of this instrument, will be found at p. 202 of this volume.) It is applied with the greatest ease, and permits of a freer application of the acid, than the speculum. —Ed. Retrospect.]

How often will it be necessary to apply the acid ? The answer to this question will depend upon the nature of each case. When there is only one vascular tumour, or a limited amount of disease, one application, pretty freely made, will suffice ; but in cases where there are several tumours, or points of disease, as many operations will be needful.

It is necessary for me to say a few words regarding the amount of suffering produced by the application of the nitrci acid. Most persons naturally connect *pain* in their imagination with the use of any caustic, arguing from the effects of the agent on the integument. Doubtless, the various caustics which are used cannot be applied to the skin without causing much suffering; but is far otherwise with the mucous membrane, especially that of the rectum. The general and immediate effect of the acid in most cases, is to cause merely a slight smarting or warmth, which goes off very speedily. In some instances, actual pain of a somewhat severe character is produced, especially when the surface touched is close upon the verge of the anus. There are however instances of hemorrhoids and prolapsus of long standing, where the parts have become so insensible, that the patients actually do not know when the acid is applied. The reason why I prefer applying it through the speculum is, that the proceeding is perfectly painless, patients having asked me the question whether the acid has been applied or not when the operation is quite finished. It is however a very different matter if any portion of the caustic comes in contact with the sensitive skin of the anus. Should such occur, the pain will be very severe, and will last for hours: hence the obvious necessity of taking the utmost precaution against this.

I can truly say that it has never occurred to me to witness anything like a fatal, or even a dangerous result, after having had a large experience of this remedy. In one case of a patient, who was most anxious to be cured by one operation, I applied the nitric acid much more freely than usual, and produced great suffering for two or three days, with the effect, however, of making a good cure. In another instance, I heard, but was not a witness of the fact, that copious bleeding followed the use of the acid. In a third instance, which occurred very lately, a young lady was treated with the nitric acid for a florid pile. Severe and unaccountable suffering was produced for many days. On making a careful examination, at the end of this period, I discovered a small ulcer, situated at the posterior verge of the anus, and exquisitely painful; suitable remedies relieved the pain in a few hours. On inquiring more minutely into the particulars of this case, I have reasons for believing that this ulcer existed before I applied the acid, and that some of the caustic came into contact with the sore, and produced the most severe suffering. The existence of a painful ulcer or fissure, in conjunction with hemorrhoidal tumours, is not unfrequent; and an examination, with the view of ascertaining this point, should be instituted, especially if it is found that the patient complains of more than usually severe pain, when at the closet, and lasting for some time subsequently. It is needless to mention that the application of the nitric acid to the rectum, when this does exist, must not be thought of.

I have now and then met with cases where retention of urine, and pretty smart bleeding, have occurred after the free use of the nitric acid, but never sufficient to cause me anxiety, and they are symptoms which are easily met.

It is not necessary to confine patients to their bed after the acid has been applied, and this is one of the reasons why the remedy is so desirable, many patients having neither the time nor inclination to submit to an operation which may keep them from their business or pleasures for a fortnight or more. (pp. 38–48.)—*Mr. H. Smith.*

162.—*A New and Rational Explanation of the Diseases peculiar to Infants and Mothers: with Obvious Suggestions for their Prevention or Cure.* By Thomas Ballard, Esq., M.R.C.S., L.S.A.—*London*, Churchill, 1860. pp. 128.

[The proposition which the author in this work endeavours to prove is, that "the exercise of the instinctive act of sucking, under circumstances unfavourable to the infant obtaining food, is a principal source of disease to infants and puerperal women."]

Dr. Brown Séquard, in his 10th Lecture, published in the 'Lancet,' says, "The excitation of the nerves of taste produces an abundant reflex secretion of gastric juice, and also a flow of bile and pancreatic juice in the bowels." The act of sucking being, par excellence, the mode to induce this excitation, it is obvious that these juices must be secreted abundantly during the exercise of this act; and if at the same time a supply of food is received into the stomach, the solvent power of the gastric juice is exerted upon it, and the first process of a healthy digestion results; but should the *sucking* have been *fruitless*, or partially so, it will in all probability have been *forcible* and *prolonged*, and therefore attended by an excessive reflex secretion of gastric juice, which, not meeting with a proportionate supply of food whereon to expend its solvent power, acts upon the mucous coat of the stomach and intestines, and causes various degrees of injury thereto, viz., destruction of the folds of mucous membrane in the upper portion of the small intestine, known as the valvulæ conniventes, thinning and softening of the coats of the stomach and intestine, and, as an extreme effect, complete destruction of the posterior wall of the stomach itself.

The evidence of this process of injury to the intestinal canal being in operation, is the presence of those symptoms which immediately result from it, viz., abdominal pain, and the frequent and green stools to which sucking infants are so liable. A persistence of this morbid state causes many of those ailments which are usually attributed to Teething, but which are really remote effects, dependent upon the imperfect development of the various tissues of the body, which necessarily results from the gradual injury inflicted on the organs concerned in the first and most essential processes of digestion: these are exemplified in the skin by erythema, urticaria, and eczema; in the system generally, by defective appetite, anæmia, tardy growth of the whole body, and general debility; and probably the establishment of that condition of the system known as strumous; in the osseous system by imperfect development of the bones, giving rise, in the case of the long bones, to crooked extremities, and in the short bones to those various forms of carious disease which are apt to succeed to a very trivial local cause; and by the tardy growth of the teeth; in the nervous system by laryngismus stridulus, convulsions, cerebral congestion, and hydrocephalus; in the abdominal viscera, by occasionally causing invagination of the intestine, or eventually producing the morbid condition known as tabes mesenterica; in short, all the diseases of infancy in which we can recognise a failure of the processes of growth and nutrition, may fairly be attributed to FRUITLESS SUCKING, as a principal, if not the chief cause. (p. 1.)

[Dr. Budd in his admirable work observes:]

"In infants the softening of the stomach is found unconnected with organic disease of other organs much more frequently than in adults, because in them the functional gastric disorder, which may be excited by teething or other causes of disturbance, rapidly exhausts the strength, causing a state of collapse, and thus proving fatal of itself."

Neither Dr. Wade nor Dr. Budd attempt to assign a cause for these morbid changes in the intestinal canal of the infant; but I think I shall be able to show that they may be attributed to the action of the excessive secretion of gastric juice, which results from the persevering efforts which are made by an infant sucking, to obtain its food, when placed in unfavourable circumstances for doing so.

The state of fatal collapse alluded to by Dr. Budd, is probably more frequently attended with entire destruction and consequent perforation of the posterior wall of the large end of the stomach than has been generally noticed, and I suggest that it is the cause of death in many of those cases of "infants found dead in bed;" of the nature of which cases the following notice, extracted from the 'Lancet,' of February, 1855, gives some idea:—

"These lifeless little bodies are discovered in at least ninety-five instances out of every hundred, after three o'clock in the morning; not one out of a hundred of such bodies is discovered dead between nine and twelve at night. An experience of fourteen years in a coroner's jurisdiction, embracing between eight and nine hundred thousand souls, has established the accuracy of these statements, by proofs which admit of no dispute. Equally true is it, that out of hundreds of examples of infants found dead in dead, only two instances have been seen in which the proof was conclusive, that the little creature had been destroyed by the pressure of persons who had been lying with them in bed, even in one of these the question might have been fairly raised. The probable explanation of these cases is the following:—The infant is accustomed to fruitless sucking, and has, in consequence, suffered for some time before, with frequent and green stools, the stomach and intestines being consequently thinned and softened; after midnight there is no prospect of food, if the mother's breast can be reached it has been previously drained, and her nervous system requires the recruiting influence of rest and sleep to enable it to secrete more milk, the infant's cravings are not satisfied, it sucks its thumb or fingers violently, or perhaps it is supplied with the *sugar teat*, with the view of pacifying it to sleep, but under any of these circumstances, the little sufferer sucks on in search of food, none is obtained; the gastric and other juices attend as usual, reflexly to their summons, and meeting with no food on which to expend their powers, complete the task of destruction they have been long engaged upon; the stomach is perforated—and instant death results. (p. 3.)

[The author then passes on to trace individual diseases to excessive and fruitless sucking. His views on *thrush* are explained in the following short paragraph:]

The true nature of the disease, I suppose to be, that the delicate mucous membrane of the mouth becomes inflamed, in consequence of excessive sucking, and that an inflammatory exudation results, which, from its situation, being warm and moist, affords a very appropriate nidus for the growth of the oidium which is found so universally growing under circumstances equally favourable to its nature. The accompanying enteric symptoms coincide with those I have previously described as resulting from fruitless sucking. The best confirmation I can adduce in proof of the correctness of this opinion, is the fact, that in all instances where I have been able to make my views of the cause of the disease understood, no case of thrush has occurred; and, in addition to this, the relation of some cases of which I have notes, where I have successfully treated the disease in accordance with them. (p. 13.)

[How is the infant to exist for three days, if, as is generally the case, the mother has no milk for that period? Mr. Ballard answers this question by relating the following case:]

A healthy woman gave birth to her ninth child at the full period. The nates presented, and the accoucheur being young and timid, the delivery was not effected with sufficient rapidity to save the life of the infant, which was exceedingly well developed. Having obtained permission to examine it, I found the stomach full of a soft pultaceous substance, somewhat resembling apple-sauce; the capacity of the organ was equal to four fluid drachms. The small intestines contained a large quantity of a similar substance, rather brighter in colour, and apparently more coagulated; the colon contained the peculiar matter known as meconium. Is it not reasonable to suppose that these substances were natural to the situations in which I found them, and that a similar condition exists in the intestines of all infants? and may we not suppose that an infant would not require milk until these substances had been disposed of? (p. 113.)—*Mr. T. Ballard.*

[In conclusion, we may say, that this little work is full of original matter, and is evidently written by a man accustomed to think for himself. We confine ourselves to giving an author's views, and leave our readers to form their own opinions as to their truth or error.]

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